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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY

Edited by Glyn Daniel

Vol. XXXII No. 128 DECEMBER 1958

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Published at Ashmore Green, Newbury, Berks., England

Price: Single Copies 7s 6d—Annual Subscription 30s (or 5 dollars)

ANTIQUITY was founded in 1927 by Dr O. G. S. Crawford

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ANTIQUITY is published quarterly on the 1st of March, June, September and December.

The ANNUAL SUBSCRIPTION (including postage) is ONE POUND TEN SHILLINGS Sterling, or FIVE DOLLARS, *payable in advance to* ANTIQUITY, Ashmore Green, Newbury, Berks., England. SINGLE COPIES may be obtained for 8*s* (post paid).

Everyone wishing to subscribe, or to obtain single copies, is asked to *write direct* to the Publisher, H. W. Edwards, Ashmore Green, Newbury, Berks., England. Tel. Thatcham 2105.

MSS (whether in the form of Articles or Notes) offered for publication should be sent to Dr Glyn Daniel, St. John's College, Cambridge, England. All MSS should be typewritten. The Editor is not responsible for the opinions expressed by contributors.

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BOOKS for REVIEW should be addressed to the Editor of Antiquity, St John's College, Cambridge, England.

Offprints can be purchased from The Whitefriars Press Ltd., 26 Bloomsbury Way, London, W.C.1. They should be ordered *at least three weeks before* the date of publication; in confirming offprint orders the Press will supply details of approximate costs.

BINDING: John Bellows Ltd, Printers and Publishers of Eastgate, Gloucester, will continue to assist those wishing to bind their copies of Antiquity. Binding cases only are 8*s* 6*d*; the cost of binding each volume is 15*s* (including case and return postage). Please write direct to John Bellows Ltd.

Antiquity

VOL. XXXII No. 128 DECEMBER 1958

Editorial

THE Hamburg Congress, held in the last week of August, is still fresh in the minds of the 750 people from all over the world who attended it. It was the third International Congress of Prehistoric and Protohistoric Sciences since the war, and it was delightful to find so many representatives from eastern European countries there and to feel that at last a completely free exchange of views on antiquity was taking place. The whole congress, described in some detail in the pages that follow (pp. 247–52) was a great success, due in no small measure to the inspiration, hard work and imperturbable good humour of the President, Professor Dr Gerhard Bersu. We look forward to the next congress, which is to be in Italy in 1962 under the presidency of Professor A. Blanc.

In his report on the Congress, Mr Powell draws attention to the remarkable display of archaeological books arranged in the foyer of Hamburg University by Dr Rudolf Habelt of Bonn. Dr Habelt tells us that it contained 780 books and 200 periodicals all published during the four years since the Madrid conference in 1954, and that his firm, which already puts all archaeologists seriously in its debt by the publication of his *Monatliche Bibliographie*, will issue a full catalogue of the books displayed at Hamburg. It is a sobering thought that with the growth of archaeology in complexity and general interest there may well be twice as many books produced in the four years that lie between us and the Rome Congress.

The books given away free to Congress members may be purchased by individuals and libraries who were not represented. Of particular value to all will be the *Neue Ausgrabungen in Deutschland*, edited for the Römisch-Germanische Kommission by Dr Krämer, which compares in a general way with the *Recent Archaeological Work in Great Britain*, edited by Mr Bruce Mitford and published by Routledge and Kegan Paul in 1956. What a pity we have not at this moment comparable books dealing with post-war work in other countries such as Ireland, Spain, France, Italy and Czechoslovakia, for example. The need for such a book for France is emphasized by the appearance of *Monuments et trésors de la Gaule* by Henri-Paul Eydoux (Plon, Paris, 1958). This is an attractive little book, beginning with Vix and Entremont, and going on to discuss nine Gallo-Roman sites from Bavai in the north to Saint-Blaise, Glanum, and Montmaurin in the south, but it does not justify its sub-title of ' les récentes découvertes archéologiques '. We want another volume, and a much larger one, on the work done in pre-Roman Gaul since the war.

The year 1958 would have been particularly appropriate for the publication of such a book on French archaeology. It is the fifteenth anniversary of the publication of the first volume of Joseph Déchelette's great *Manuel d'Archéologie*, and the sixtieth anniversary of the first essay into print—albeit about anomalies in insects—of the doyen of French archaeology, l'Abbé Henri Breuil. The Abbé Breuil, to whom we wish many more years of

life and work, celebrated his eightieth birthday last year and on this occasion a bibliography of his work (with an introductory essay by Monsieur Lantier) was prepared and published by three of his pupils and friends, namely Mademoiselles G. Henri-Martin, S. de Saint-Mathurin and D. Garrod. Under the title of *Hommage a l'Abbé Henri Breuil pour son quatre-vingtième anniversaire* (Paris, 1957), this is indispensable to all libraries and students and gives an amazing conspectus of the things one great archaeologist has written about in a long lifetime of work, from the Bronze Age in the Aisne and Altamira, to megalithic art and Rouffignac. (Obtainable from E. J. Brill of Leiden.)

Rouffignac is now coming again into the news as more and more archaeologists are able to visit it, and see for themselves the disputed paintings and engravings about which there was such a furore when they were first ' discovered ' by Professor Nougier and Monsieur Romain Robert in 1956. The cave has been equipped with electric light and a railway, and by Christmas (or at latest February) it will be possible to take a seat in a miniature train and travel along the winding galleries while lights pick out the paintings in the Galerie Henri Breuil and elsewhere. The speech of Professor Nougier describing the discovery of the Rouffignac paintings and engravings has now been made available in the *Compte rendu* of the Poitiers-Angoulême Congrès Préhistorique de France, the Abbé Breuil has published a full account of his analysis of the site (*Gallia*, 1957, fascicule 3, 1 ff), Nougier and Robert have written a book which is now available in French and English, and Pierret has published his *Précisions sur Miremont*. We print below a review of the Nougier-Robert books and of Pierret by Dr Dorothy Garrod, who brings to the question of Rouffignac a life-time of expert study of Palaeolithic art, and whose views will command the highest respect.

Miss Garrod served on the International Commission which studied the Glozel forgeries and it is worth thinking back over Glozel at the present moment. *L'affaire Glozel* and *l'affaire Rouffignac* have much in common, most of all in the passionate over-advocacy of distinguished scholars. Many French archaeologists were convinced of the authenticity of the Glozel finds, as was most of the learned world convinced of the authenticity of Piltdown Man. The late editor of ANTIQUITY suspected Glozel from the beginning and ended a remarkable article in the second number of this journal with the words: *We conclude by repeating our opinion that the inscriptions, the engravings, and the majority of the other finds are forgeries, and that those who believe in their authenticity have been the victims of a hoax* (ANTIQUITY, 1927, 181).

L'affaire Rouffignac, like all tales of disputed antiquities, has four aspects. There is first—though it ought to be last—the subjective judgment of expert art-historians, archaeologists and artists. Secondly there is technical evidence. Thirdly, there is the history of the site, particularly the gossip of the last twenty years. And lastly, there is the treatment of the issue by the protagonists. It is this last issue which comes first in the eyes and mind of a jury in an English court—the demeanour and character of defendants and witnesses. If any book could do harm to the defence it is *Rouffignac ou La Guerre des Mammouths*, as Miss Garrod so clearly shows.

Four members of the Cambridge University Speleological Society visited Rouffignac on 31 March, 1939; they spent a considerable time in the cave and were themselves keen, and fresh from examining the known painted caves near Les Eyzies. Yet they saw nothing, as Martel had seen nothing during the eighteen hours he spent in the cave in 1893. It does need saying clearly that it is not difficult to see the Rouffignac paintings; they are clear and boldly done. It seems highly improbable that intelligent visitors from 1893 to 1939 could have missed them—if they were there. The evidence of people like Martel, the 1939 Cambridge speleologists, as well as the clear assertions of recentness made by men like William Martin and Bernard Pierret and Severin Blanc—men who would only have been too glad to dis-

cover an authentic Palaeolithic cave in the Dordogne—suggests that Rouffignac is, to put it mildly, a 'salted' site. It looked that way to the present editor of ANTIQUITY in 1956, and a further visit in September of this year suggested that one might say of at least some of the Rouffignac paintings, in Dr Crawford's words, that ' those who believe in their authenticity have been the victims of a hoax '. We may be proved wrong; what is certain, as Dr Garrod argues, is that there should be an impartial non-archaeological enquiry into the history of Rouffignac. We ourselves have been convinced of this ever since it was realized that some of the paintings ' discovered ' in 1956 were clearly shown in pl. XVI of Pierret's *Le Périgord Souterrain* published in 1953. Pierret now says that they ' appeared ' between September and December 1948 and this statement alone should be rigorously examined before anyone writes any more about the paintings as authentic and Palaeolithic.

Glozel and Piltdown still call up strained smiles, but we can all laugh over the Kensington Stone, and Professor Wahlgren has provided us with a new opportunity to do so. It is rare for a University Press to discover that it has published a best-seller; rarer still to find that the best-seller is a detective story and competes with Agatha Christie, Ellery Queen, Georges Simenon and Raymond Chandler. Yet Weiner's *The Piltdown Forgery* was such and was rightly described by the Ellery Queen organization as ' the best detective story of 1956 '. And now, the University of Wisconsin can congratulate itself on publishing what is surely one of the best detective stories of 1958, namely Erik Wahlgren's *The Kensington Stone: a Mystery Solved.* This fascinating book is discussed below (pp. 264–7); suffice it to say here that we read it, and Raymond Chandler's *Playback*, on the boat from Harwich to Esbjerg on the way to the Hamburg Congress, and while yielding to none in our admiration of Chandler, yet Wahlgren won the day. There are no hard-faced dames in oyster-white upholstered convertibles in *The Kensington Stone*, no pockets full of bourbon and rye, but the story which Wahlgren tells—and tells so well—will satisfy the most avid detective story reader.

And if it should be thought that there is too much in these Editorial notes and in this number of ANTIQUITY about forgery and detection, our excuse is that it is the December number and that Christmas is the season of fun and games, *farces et atrapes*, false red noses and long white beards. Whenever I think of Glozel I think of Mistinguett's cruel sketch in the Moulin Rouge when, wearing a long white beard she excavated busily, triumphantly unearthed a chamber pot, and deciphered on its sides the names of several distinguished archaeologists.

ANTIQUITY, 1959

Readers are reminded that subscriptions to ANTIQUITY for 1959 (Volume XXXIII) are now due and, if not already arranged by Bankers Order, should be sent to the Publisher of ANTIQUITY, H. W. Edwards, Ashmore Green, Newbury, Berkshire, England. Among articles which it is hoped to publish in 1959 are: *The Gundestrup Bowl: a Reassessment*, by Dr O. Klindt-Jensen; *The Jericho Excavations*, 1957–8, by Dr Kathleen Kenyon; *New World Origins*, by Chester Chard; *The Nuraghi of Sardinia*, by Professor G. Lilliu; *Over-taxation and the Decline of the Roman Empire*, by Professor A. H. M. Jones; *The Motif of the Grazing Horse in Palaeolithic Art*, by Dr Dorothy Garrod and Mademoiselle de St Mathurin; *Archaeology and the Ordnance Survey*, by C. W. Phillips; *The Hoard of Celtic Metalwork from St Ninian's Island, Shetland*, by Professor O'Dell, Mr Bruce-Mitford, Mr R. B. K. Stevenson and others.

Early Shipping in the Near East

by R. D. BARNETT

In this article, Dr Barnett, Keeper of Western Asiatic Antiquities in the British Museum, discusses the evidence for ships and shipping in antiquity in Mesopotamia, Egypt, the East Mediterranean, and the Aegean.

OUR knowledge about ancient shipping in the Near East is fragmentary in the extreme. To reconstruct it we have to rely on perfunctory sketches or models which give us a very inadequate idea of the progress of ancient shipbuilding. Most conclusions are, therefore, for the present, purely tentative.

MESOPOTAMIA. Here, as elsewhere, primitive communities evolved primitive methods for conveying themselves on the water, though direct evidence of these methods is not found in the earliest levels excavated, nor in works of art associated with them. However, such primitive methods as air-filled skin floats, surviving into later periods, are depicted on Assyrian monuments of the 9th century B.C., where soldiers assaulting a city are using goatskins for crossing the river.[1] It is thought that such skins may well have been carried as part of the Assyrians' military equipment, as was the habit of the Mongols in medieval times.[2] An Assyrian soldier is shown holding the neck of the skin between his teeth, not, I think, to keep it inflated, as Hornell suggests, but to free his hands for paddling. Other reliefs are said to show Assyrians using such skins for fishing,[3] but this is doubtful. There can be little doubt that this primitive device, and certain other methods of early water transport to be described, which still survive today in Mesopotamia, were developed very early because they were then the only ones suited for local conditions.

The next step after the inflated goatskin-float is to join several of them together, cover them with a deck, and make a raft. Such buoyed rafts, called in Babylonian *kalakku*[4] and still called in Arabic, *kelek*, are still used on the Tigris for floating large or small cargoes downstream where any other craft would easily come to grief in the rapids. Layard used such rafts a little over a century ago to convey his sculptures to Baghdad and Basra, where they were loaded on to sailing ships. Keleks can be from 10 to 15 ft. square and be buoyed by fifty to a thousand skins. After they have reached their destination downstream the skins are deflated, washed, tanned and taken home by land on donkey-back to Mosul or Takrit, the woodwork and lashings being sold. Herodotus[5] saw them in use, but confused them with that other common type of primitive boat, the coracle (called by Assyrians *quppu*, in modern Arabic, *quffa*), consisting of a basket-work frame (sometimes covered with skins) covered with bitumen. For propelling this vessel, queer racquet-shaped paddles were used in antiquity and continued in use until Sassanian times.[6] Similar coracles are still used today in some rivers of Western Ireland and South Wales. In Assyria, the *quffas* were large enough to carry a complete chariot with its crew.[7]

Other Assyrian sculptures show us the Babylonian marsh-dwellers and Elamites using

[1] Budge, *Assyrian Sculptures in the British Museum*, pl. XIII.

[2] Hornell, *Water Transport*, p. 8. The inflated skin is called in Assyrian *maskêru*. Salonen, *Die Wasserahrzeuge in Babylonien*, p. 71.

[3] Hornell, p. 7, quoting Mansell photo 430.

[4] Salonen, p. 66–8; Hornell, p. 26 ff.

[5] I, 194. See Salonen, pp. 71–4.

[6] They are seen on the great hunting scene carved at Taq-i-Bostan.

[7] Budge, *Assyrian Sculptures in the British Museum*, pls. XXI, XXII.

elongated *boats of reeds* (in Assyrian, elip urpati), in which they try to make their escape from the Assyrians. Such boats are mere mastless canoes; they are still made of reeds, basketry and bitumen at Hit on the Euphrates.[8] A barge derived from such a boat, called *maqurru* in Assyrian, was used for the ritual processions of the Gods by water; it appears to be mastless, but has each end raised high above the water line and tied back,[9] and it was no doubt in use from the very earliest Sumerian times, since from it is derived the cuneiform sign for ship. Illustrations on cylinder seals carry it back at least to the Jemdet Nasr period, about 3200 B.C. (PLATE XXI, *a*).

About 3000 B.C. we find a canoe-shaped boat used in the marshes for hunting buffalo[10] and in the Royal Cemetery at Ur was found a silver model of such a canoe, with seven thwarts and six pairs of paddles[11] (PLATE XXI, *c*). A similar canoe-model in bronze has been recently found at the Phoenician site of Byblos.[12] Fair-sized clay models of similar canoes, some on a framework of withies, and containing pots, were placed in tombs at Ur in the Sargonid period, about 2250 B.C., perhaps for the use of the dead.[13] This type of canoe corresponds to the type of boat still called a *mashuf* on the Lower Tigris, made of basketwork covered with a frame of bitumen.

The Euphrates is navigable as far as Jerablus, and the Tigris as far as Baghdad; it seems likely, therefore, that *sailing* was developed and used in Mesopotamia at an early date, though the evidence of texts and pictures is wanting. The digging of canals in the 3rd millennium may have given less scope than that offered by the broad rivers for the use of sail; but sailing ships were certainly known in the middle of the 4th millennium, since a clay model of a small boat of oval form with a single mast supported by three stays has been found at Eridu (PLATE XXI, *b*).[14] It is, of course, little more than a single-masted *quffa*. It has also been pointed out that the remarkable wealth in precious stones and metals found in the Royal Cemetery at Ur about 2500 B.C. may be a result of trade with the Indus valley, though no details of the routes by which this was maintained are known at present.[15] Contact with the Indus Valley is proved by the presence at Ur (once a seaport) of Indian seals. By the end of the 3rd millennium at Ur there is evidence that copper and ivory were brought from the Persian Gulf to Bahrain as an entrepôt and then transhipped to Ur.[16] In what kind of boats this Indian merchandise made its journeys from India to the West is a matter for future discovery, since almost nothing is known about them; a graffito on a potsherd from Mohenjo-Daro shows a sailing boat with upturned bow and stern[17] and a steering paddle. A seal shows a mastless river boat with central cabin.[18] But the earliest representation of real sea-going ships in India is of the 10th century A.D.

The only piece of information which may concern early sailing craft in Western Asia is a graffito on a stone vase-fragment recently found at Sakca-gozu,[19] in North Syria. Beside the picture of a fox is drawn an object which might be a sailing ship with sail furled; but it might also be interpreted as representing a tree!

[8] Hornell, p. 56.
[9] Salonen, pp. 12–19 and pl. III, 2, 3.
[10] B.M. 118466, *British Museum Quarterly*, vol. II, pl. VI, *b*.
[11] Woolley, *Royal Cemetery*, pl. 169.
[12] Dunand, *Byblos* II, pl. LXIX.
[13] They contained paddles or punt-poles—Woolley, *Royal Cemetery of Ur*, p. 145. One such canoe is now at Birmingham.
[14] Lloyd, *Illustrated London News*, 11 September, 1948.
[15] C. J. Gadd, ' The Royal Graves of Ur ', *The Listener*, 3 March, 1955.
[16] Leo Oppenheim, 'The Seafaring Merchants of Ur ', *Journal of American Oriental Society*, 74, 1954.
[17] Mackay, *Further Excavations at Mohenjo-Daro*, Delhi, 1937, 2, p. LXIX.
[18] Mackay, *ibid.*, pl. LXXXIX, *a*. See Wheeler, *The Indus Age*, p. 60.
[19] V. Seton-Williams, *Iraq*, XII, p. 118.

Digby [20] thinks that sail was invented in the Nile Valley, where the constant north wind favoured its use upstream while the current was enough to carry ships in the opposite direction.

EGYPT. A representation of a sailing boat with a sail, perhaps of matting, occurs on a predynastic vase of Amratian style from middle Egypt in the British Museum, dated perhaps to about 3500 B.C.[21] (FIG. 1) Its high, vertically-raised extremities recall the pictures of boats with central mast, but no sail, roughly drawn on the rocks of the Eastern Desert.[22] But the position of the mast off-centre recalls that of the symmetrical barges with upturned ends and mast ending in a fork or crutch, represented in the famous battle scene on the Jebel-el-Arak knife; these barges are thought to be those of Semitic invaders of predynastic Egypt. This forked mast could be a crutch for a steering paddle, as used by local vessels in the famous scenes of shipping at Punt.[23]

In Egypt, the reed-boat was familiar from predynastic times onwards, and was used especially for fishing and fowling.[24] When in due course it evolved into a proper form of

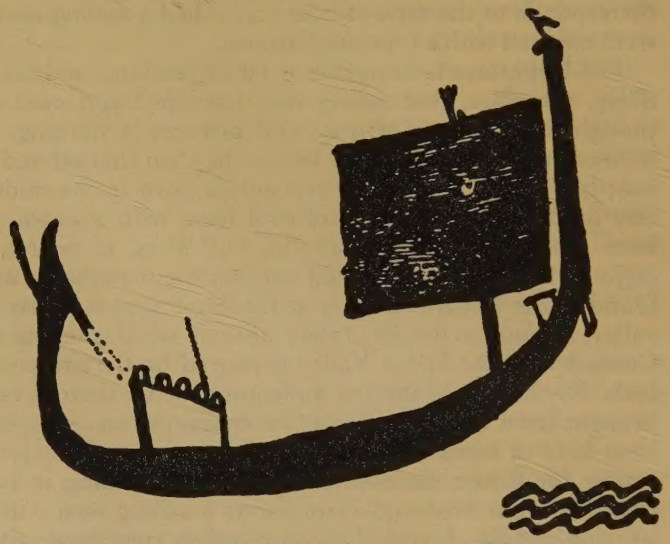

FIG. 1. Egyptian sailing boat from pre-dynastic vase of Amratian style, dated c. 3500 B.C. (British Museum.)

ship, its sail was mounted on a two-legged mast to distribute the strain. This at least was the practice until the 5th Dynasty (about 2500 B.C.) In the 6th Dynasty (about 2300 B.C.) we find that a ship 102 ft. long could be built in seventeen days. Such ships had triple steering oars and no keel, and they were held together by a longitudinal truss of rope twisted to keep them taut.[25] We may expect quite new and important information about the types of early Egyptian boats from the example,[26] buried by the Pyramid of Cheops, and newly discovered. The only information at present available shows that it has a centre-board keel, a cut-back stern post, and the stern conventionally shaped like a papyrus bundle. It is decked, and crew and goods may have sat on top of the deck.

[20] A. Digby in Singer and Holmyard, *History of Technology*, I.

[21] B.M. 35324. Childe, *New Light on the Most Ancient East*, fig. 51. Though usually called boats, the centipede-like objects on predynastic vases are possibly not boats, but stockades! (Information from Mr T. G. H. James.) O. G. S. Crawford, however, believed them to be boats, with a branch of a tree in full leaf used as a sail.

[22] H. Winkler, *Rock Drawings of Southern Upper Egypt*, I, pl. XXXVII.

[23] See below and note 30.

[24] Good illustrations: Winlock, *Models of Daily Life in Ancient Egypt*, pl. 52, *a*.

[25] From the Tomb of Ti (Schäfer-Andrae, *Die Kunst des alten Orients*, pl. 251).

[26] *I.L.N.*, 19 June, 1954.

In the records of the Middle Kingdom (2160–1788 B.C.) we hear [27] of really large ships 180 ft. long with a beam of 60 ft., a ratio of one to three, which is similar to that of medieval merchant ships. The mast is now a single pole instead of a biped, and the rudder is reduced to a single paddle attached abaft the cabin at the stern. Ceremonial funerary boats, however, retain one rudder each side, rotatable at 45° to the water.[28] A square sail is now extended by a lower yard or spreader, supported by diagonal ropes or ' lifts ' to save the sail fabric from tearing. In the absence of a true keel, a line of carefully fitted planks ran from bow to stern, fixed together by double wedges of sycamore.

Whether these ships were all Egyptian-built, or built to Egyptian orders abroad, is obscure. The Egyptians at this date make mention of a type of craft called *kbnt*, ' ships of Byblos ', but whether this means ships built there, or built for the ' Byblos run ', is not known. The latest authority to write on the subject[29] believes them to be wholly Egyptian; but I believe the Egyptians were never at home on the open sea, only on the rivers, and had their ocean-going ships, if not actually manned by foreigners, at least built by foreign (i.e., Syrian and Phoenician) shipwrights.

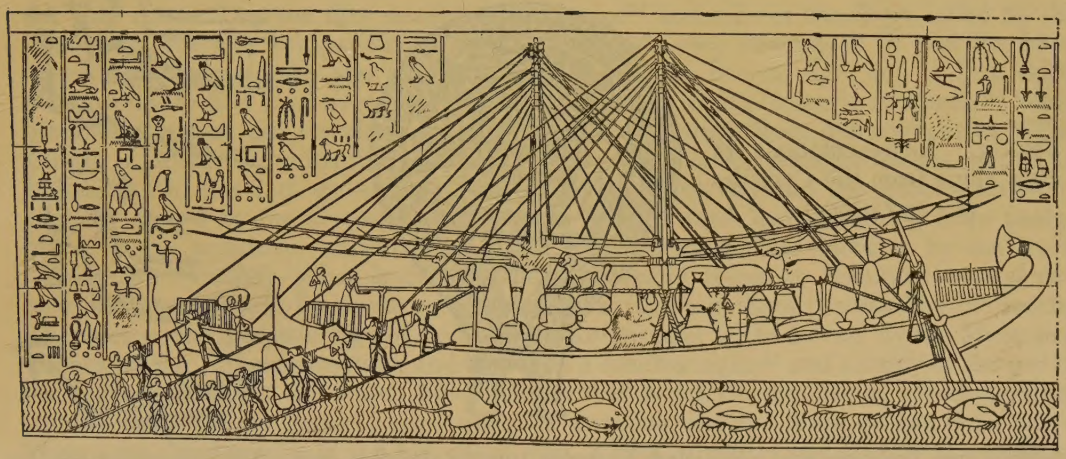

FIG. 2. Ships of Queen Hatshepsut at Der-el Bahri (*see below*).

In the 18th Dynasty, in 1480 B.C., Queen Hatshepsut made her famous expedition to Punt or Somaliland and depicted the event on the walls of her temple at Der-el Bahri.[30] Her ships were apparently about 70 ft. long, 17 ft. or 18 ft. wide, and 4 ft. or 5 ft. deep. They used a truss-system identical with that of the 5th Dynasty. The differences to be noted were that the mast and sail were now amidships, the mast being a single pole, the sail very wide, and the sail's lower yard being supported by ' lifts ' passing through ' bees ' lashed to the masthead similar to an eighteenth century bowsprit.

The upper yard consisted of two spars lashed or ' fished ' together to economize wood; the spar naturally having to taper at either end, it was realized that this could be best achieved by joining two spars made from tree-trunks at their thicker end (FIG. 2). This was a method practised until the Middle Ages in the Mediterranean.) The sail was manipulated by two braces leading to the upper yard, but propulsion was also provided by means of

[27] Digby, *loc. cit.*
[28] Digby, *loc. cit.*
[29] Säve-Söderbergh, *The Navy of the XVIIIth Egyptian Dynasty.*
[30] Naville, *The Temple of Deir-el-Bahri*, pt. III, pls. LXIX–LXXV.

thirty oarsmen. In the relief they are archaistically shown as if paddling instead of rowing. There are two great steering-paddles lashed to a vertical post manipulated by a vertical tiller.

The Tomb of Tutankhamen contained a beautiful model of a fully-rigged sailing ship. It shows a craft very similar to that of Hatshepsut, one hundred years earlier (except that a heavy transverse beam is used for the steering oars.)[31] Another model from the same Tomb[32] shows a boat without a sail which has been hailed as being apparently equipped with a distinct external keel. If this is correct, we have a complete novelty in Egyptian shipbuilding practice, perhaps due to influences outside Egypt, as we have had evidence hitherto only in the Cheops boat for a centre-board keel.

THE AEGEAN. What meanwhile was taking place elsewhere? We must recognize in the Aegean the appearance of new elements in the history of shipping. We are no longer dealing with river craft, but with vessels designed for the open sea. The earliest illustrations show in use in the Cyclades,[33] long canoes without a sail, having a high prow decorated

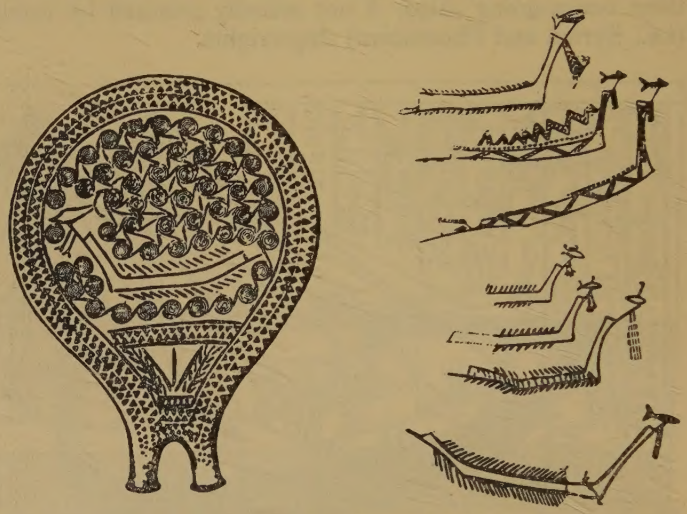

FIG. 3. Representations of early ships from the Cyclades (*see text above*).

with a fish emblem and propelled by some twenty paddles (FIG. 3).[34] Somewhat similar craft are to be seen in the hieroglyphs of the famous Phaistos disc, and a later Cretan variant is seen on a Minoan seal from Mochlos,[35] where the prow takes the form of a horse's head reversed (FIG. 4). On Minoan seals we now meet also ships driven by both sail and oars, ships both long and rounded in outline.[36] Their maximum number of oars is fifteen a side, making a thirty-oared boat, a number which agrees well with such ancient stories as that of the thirty Minyans described by Herodotus.[37] When, however, mythology describes

[31] Penelope Fox, *Tutankhamen's Treasure*, pl. 58.

[32] G. S. Clowes, *Sailing Ships* (Science Museum, S. Kensington, 1930), p. 25, apparently referring to Carter, *The Tomb of Tutankhamen* III, pl. LXI (*a*).

[33] Köster, *Schiffsfahrt und Handelsverkehr*, figs. 5, 6.

[34] The remarkable graffiti at Hal Tarxien, Malta, described by Mrs Woolner in *Antiquity*, June 1957, depict oared boats with one mast and high prows, apparently of the 16th century B.C. Some of these recall the Cycladic representations.

[35] Seager, *Mochlos* (fig. 52).

[36] Marinatos, 'La Marine Créto-Mycénienne', *Bulletin de corresp. Héllénique*, LVII, gives an invaluable catalogue of 69 illustrations of ships, mostly from gems. A type of ship called 'Keftiu-ship', i.e., some kind of Aegean craft, was either built or repaired in the dockyards of Thothmes III. Glanville, *Aegyptologische Zeitschrift*, 68. Säve-Söderbergh, *op. cit.*, p. 47.

[37] iv, 148.

FIG. 4. Minoan seal from Mochlos (*see p.* 224).

the ship manned by the fifty daughters of Danaos[38] and tells of the *Argo*, sailed by the Argonauts, the fifty companions of Jason, it suggests very strongly to us that the fifty-oared ship or *penteconter* was already known in the late Bronze Age; and in fact, ships with twenty to fifty (and even 120) oars are mentioned in the Homeric catalogue.[39] What other information can we glean about Mycenean Age ships from literary sources? The Linear B tablets are unhelpful; but an important gloss in the Amarna letters, written in Babylonian in the 14th century B.C. and found in Egypt, explains the Babylonian word *ellippu* (' ship ') by the Canaanite word *'anâji*.[40] This word *'anâji* has long been recognized as the primitive, uncontracted form, represented in later Hebrew by the word *'ôniyâh* (for **'anajiâ*). What is the etymology of *'anâji?* It has no Semitic root and can only be from the Indo-European root of *naus-navis* (ship), perhaps preceded by the article. This means that a certain type of ship was already brought to the Levant by the Mycenean Greeks by the 14th century B.C., and had been a familiar sight,[41] and a study of the use in the Hebrew Bible of the word *'ôniyâh* shows that it described a heavy, sea-going ship, usually in the phrase *'ôniyat Taršiš*, a ship of Tarshish, which I think we can show was a Phoenician development of the Mycenean ship. A broken vase of late Mycenean date from Melos introduces us to the ' long ship ', which derives its abrupt bow from the Cycladic vessels, but has a larger oarage.[42] A crude drawing on a sub-Mycenean stirrup vase from Asine[43] first shows an important innovation. This is the ram, projecting to a sharp point at the bow of the vessel, obviously as the continuation of a firmly constructed external keel. This invention was in complete contrast with the contemporary methods of shipbuilding, both of Egypt and of the ' Sea-Peoples ', and implied new methods of naval warfare. The ' Sea-Peoples ', presumably invaders from the Northern Levant, are the only other seafaring people of the time, of whom we have any evidence. If we may trust the pictures in the famous representation of the battle in which the ' Sea-Peoples ' were repulsed by Rameses II,[44] in 1190 B.C., their ships were either nearly straight at either end, and ending in lions' heads, or rose abruptly to end in ducks' or geese' heads, had a single mast with a

[38] Danaos emigrated with his fifty daughters according to legend, in a home-built sailing-ship, the first *two-prowed* vessel ever to be launched, in contrast, no doubt, to the Cycladic craft.

[39] Iliad, ii, 119–20, 509–10.

[40] Barnett, Mopsus, *J.H.S.*, 1953, p. 140.

[41] This seems confirmed by the reference to Keftiu-ships in Egypt. See above, note 36.

[42] Kirk, ' Ships on Geometric Vases ', *Annual of the British School at Athens*, XLIV, 1949, p. 116.

[43] *Ibid.*, fig. 5.

[44] Wreszinski, *Atlas z. altäg. Kulturgeschichte*, pl. 116.

crow's nest and a single sail, and an oarage of at least nine a side (FIG. 5). The steering-paddles are as in Egyptian boats. In some respects—such as the crow's nest and the use of animal figure-heads—such ships are not unlike the later representations of Phoenician ships depicted at Khorsabad[45] which have horse's heads. Indeed, one may ask whether (if these representations can be trusted) the ' Sea-Peoples ' were influenced in their ship-building by the Phoenicians or vice versa, perhaps through the establishment of provincial shipyards at such places as Tarsus or Phasēlis, drawing on the timber of the Taurus forests. We recall the horse-headed prow of the Minoan boat; but we know too little as yet of Phoenician shipbuilding to answer the question.

FIG. 5. Ships of the 'Sea-Peoples' (*see p.* 225).

THE PHOENICIANS built with wood from the forests of Lebanon, and a type of ship called ' Byblos ship ' (*kbnt*) as stated above, was familiar to the Egyptians, but what exactly this term meant—whether ships built at Byblos, or built for the ' Byblos run ', is in dispute.[46] The Phoenician port of Byblos was, of course, the great port whence Egypt traditionally obtained her timber, and it is to Byblos that in the 19th Dynasty an Egyptian emissary, Wen-amon, was sent to buy wood with which to remake the great barge of the God Amon-Ra at Thebes. The Pharaohs from Thotmes III (1504–1450 B.C.) to Amenophis IV (1370–1352 B.C.) possessed a naval arsenal or dockyard—in Aramaic, *bêt-sefinâtâ*—at Peru-nefer (Sakkara), in which Asiatics were employed,[47] and a type of ship called *silsila* (apparently known to Isaiah long afterwards) was built there.[48] A Phoenician, Barakat-Ilu, is known as the owner of a substantial fleet of trading ships.[49] Another type of Phoenician ship, as we learn from the Bible, was the *'ôniyâth Taršiš*, ' ship of Tarshish '. The term ' ships of Tarshish ' is familiar to all. But where was Tarshish ? As I have said elsewhere, there seems insufficient reason to interpret Tarshish as denoting any place but Tarsus in Cilicia.[50] What then were these sea-going ships of Tarsus ? Torr[51] points out that ' among Greeks the oars of a ship were collectively termed *tarsos*. The coincidence suggests that this city was pre-eminent in furthering the use of oars in the Mediterranean.' The clue is given by an important sculpture found at Nineveh, till recently unknown,[52] which depicts the flight before Sennacherib's attack of the Phoenician fleet in 700 B.C. from the city of Tyre, from which Luli, King of Sidon, is embarking with his family (PLATE XXII, *b*). We see two sorts of ships—a fleet in which already the distinction is made between ' round ' and ' long ' ships. The ' long ' ships are equipped with the pointed ram of Mycenean craft,

[45] Botta, *Monument de Ninive*, pls. 52–4. See also below on these *hippoi*.

[46] Säve-Söderbergh, *op. cit.*, p. 48.

[47] Glanville, *loc. cit.* (see note 36). Säve-Söderbergh, *op. cit.*, p. 37, Leibovich, *Annales du Service des Antiquités de l'Egypte*, xii, 442. N. A. Giron, *Textes Araméens d'Egypte*, pp. 13, 25.

[48] Isaiah, xviii, 1.

[49] Report of Wen-Amon (Maspéro, *Popular Stories of Ancient Egypt*).

[50] See above, note (40).

[51] *Ancient Ships* (1895), p. 2.

[52] Published first in Barnett, *Archaeology*, July 1956.

a heavy with a hurricane superstructure deck, in Greek called καταστρῶμα and a screen, on which shields are hung. The single mast has one yard carrying a square sail. The lower yard of the Egyptian XVIIIth dynasty ships of Punt is discarded; the rigging now resembles that of the Sea-People. These are biremes, having a complement of some fifteen or twenty-five oars a side in two banks, that is thirty or fifty oars, and these must be the famous ' ships of Tarshish '.[53] Torr [54] notes aptly that ' the Mediterranean is a sea where a vessel with sails may lie becalmed for days together, while a vessel with oars could easily be traversing the smooth water with coasts and islands everywhere at hand to give her shelter in case of storm. In that sea, therefore, oars become the characteristic instrument of navigation and the arrangement of oars the chief problem of shipbuilding.'

' The Homeric Catalogue (*Iliad* II, 719–720) attributes to some ships fifty oars, but to the Boeotians 120 men, that is, 118 oarsmen, if we deduct helmsman and cox. This implies an arrangement in *two rows* of thirty-two a side, formed by inserting ports for fifty-eight oars in the intervals between the tholes on ships of sixty oars. Yet the Greeks never, as far as we can see, employed sixty-oared ships themselves and had no name for them. So the invention was not theirs.' The ship with double banks of oars which might number fifty or sixty, therefore, was a Phoenician invention, and in my view, the ' ship of Tarshish ' was just the sea-going Mycenean ship, adapted by the Phoenicians to the principle of banked oars.

We can distinguish *at least* three or four types of Phoenician ships. The main division is, as we have said, ' round ' or ' long '—' round ' for merchantmen, ' long ' for warships—a division which was derived from Minoan, and continued into Roman times. The Phoenician merchantmen are easily recognized on the scene of the Phoenician navy fleeing from Tyre described above. These round ships with symmetrical ends are descended evidently from the much older Phoenician merchant-ships depicted in the Tomb of Neb-Amun[55] in the 14th century B.C., belonging to the reign of Amenophis II. They have fencing along the gunwale, intended, as in Odysseus' ship, to keep the sea out.[56]

A similar ship represented in the Tomb of Ken-Amun (XVIIIth Dynasty), known only from a none too reliable drawing, much defaced (FIG. 6).[57] However, these ships have sails and rigging much like the ships of Punt, with two yards, rounded hulls, vertical stem and stern, and a fence along the gunwale. These are the type called *gôlah* (Greek γαῦλος, or in translation, στρόγγυλος, ' round '.[58] A terracotta model of a ship of this type from Amathos[59] shows housings for the helmsman and steering oars, portholes for oars, deckboards, and four thwarts (PLATE XXIV, *a*). A vase from Cyprus[60] shows a ' round ' ship freighted with wine; a man climbs on, heaving anchor, another falls off; the ship has rounded prows, sails furled and the sheets gathered (as on the Assyrian and Karatepe reliefs) to the foot of the mast, in opposition to the Greek tradition in which they are carried to the stem and stern. The anchor is a plain stone with a hole in it (PLATE XXIII).[61]

A third type of merchant-ship used by the Phoenicians was that called by the Greeks *hippos* or ' horse '.[62] It can now be identified. Its prow ended in a horse's head and it

[53] The Assyrian equivalent term for ' Ship of Tarshish ' is *elep qarabi* ' man of war '. This passed through late Greek καράβος into English ' carvel '.

[54] *Ibid.*, p. 1.

[55] Köster, *op. cit.*, pl. 1.

[56] Odyssey, V. 256–7.

[57] Säve-Söderbergh, *op. cit.*, fig. 10 and pp. 56–8.

[58] Torr, *op. cit.*, p. 113.

[59] British Museum, Department of Greek and Roman Antiquities, A.202.

[60] British Museum, Department of Greek and Roman Antiquities, 1926, 6.28.9.

[61] The anchor is said to have been first invented by Anacharsis, *c.* 600 B.C. (Strabo VII, 39). The word ἄγκυρα first occurs in Alcaeus, 6th century B.C.

[62] Torr, p. 113.

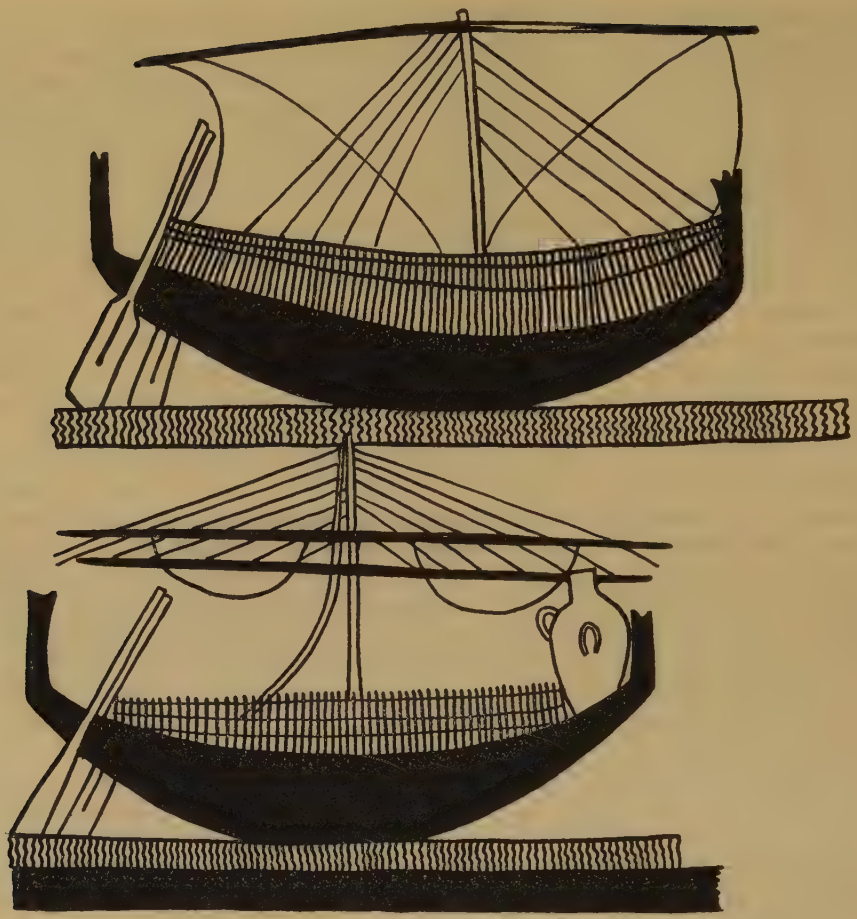

FIG. 6. Ships from the Tomb of Ken-Amun (*see p.* 227).

could be either with a mast, as on the sculptures at Khorsabad,[63] where there is a crow's nest, or an unmasted barge, as seen on the Gates of Balawat.[64] Was it inspired by the Minoan barge with horsehead reversed? In later times it seems to have died out in Phoenicia proper, but survived, chiefly for fishing, near Cadiz.[65] An illustration from Phoenician jewellery (PLATE XXIV, *b*) found at Aliseda in Spain, shows a horse's head at each end.[66] An Assyrian relief of the 7th century shows such a boat, constructed with a single horse-head and two banks of oars (PLATE XXII, *a*), used by the Assyrian King Assurbanipal to hunt lions in the Babylonian marshes.[67] Strabo mentions that the use of *hippoi* continued in the Mediterranean to the end of the first century B.C.[68] One cannot

[63] See above, note 45.
[64] King, *Bronze Reliefs from the Gates of Shalmaneser*, pl. XIII.
[65] Torr, p. 113.
[66] Melida, *Tesoro de Aliseda*, pl. opp. p. 27.
[67] Salonen, pl. XIX, 2.
[68] On the movement of the Mediterranean tradition of boat-building northwards round the coasts of Spain to Scandinavia, see the excellent remarks of Lethbridge, *Boats and Boatmen*, p. 8, and pp. 92 ff.

help suspecting that these well-built keeled ships with the characteristic animal-headed prows found their ways round the west coasts of Europe and stimulated imitation amongst the Veneti[69] and others further north.

We find on sculptures and vases illustrations of another type of Phoenician galley equipped with a projecting ram, but with only one bank of oars. Was this type introduced into Greece, or more probably, was it not originally just the Mycenean penteconter introduced into Phoenicia? Such a ship is seen on the Phoenician relief of Karatepe,[70] and also on the surprisingly similar ivory plaque from the Orthia Temple at Sparta,[71] where the man fishing, the fish and the Phoenician row of shields, exactly as at Karatepe, all recur. This type of ship lasted apparently into the 5th century, being taken as an emblem on the coins of Sidon (PLATE XXIV, *c*). It was probably the type of ship called by Assyrians *gurgurru*, by Phoenicians called *kirkarah*, by Greeks κέρκουρος, which was said to be equally suitable for warfare and commerce.[72] A good illustration is probably on a seal impression from Persepolis.[73] From the fact that a certain kind of boat plying to Tarshish was called *sefînâh* in the Book of Jonah, or ' decked ' (Neo-Babylonian *sapinatu*), we may assume others were undecked.

According to Thucydides[74] the Corinthians in about 700 B.C. were the first of the Greeks to build *triremes*, a term the meaning of which is far from agreed. If it means ships

FIG. 7. Representation of bireme from Attic vase (*see below*).

with three banks of oars, Thucydides' statement seems unlikely, since apparently, such ships were invented only in the 6th century B.C. in Ionia. Until that time, the Greeks seem (to judge from representations on vases) to have known only the bireme of two banks,[75] borrowing it from the Phoenicians, the earliest Greek representations of biremes being on colossal geometric Attic vases of the 8th century B.C. (FIG. 7). The use of the ram in battle is shown in a single decker, on the ' Aristonophos vase '.[76] The general appearance of three-banked triremes is disclosed in a terracotta model from Erment in Egypt, perhaps of the 4th or 3rd century B.C., which Dr Niels Breitenstein, Keeper of the Antiksammlingen of the National Museum at Copenhagen has kindly allowed me to publish (PLATE XXIV, *d*). The ram is shaped like a porpoise head. Along the decks shields are hung in the Phoenician manner. The construction of three banks made possible an oarage of 200,

[69] Caesar, B. G., III, 14. Strabo, IV, p. 195.
[70] Riemschneider, *Die Welt der Hethiter*, pl. 83.
[71] Dawkins, *Artemis Orthia*, pl. CX.
[72] Salonen, p. 51; Torr, p. 110.
[73] Oriental Institute Communications, 21 (1939), fig. 24. I owe this reference to the kindness of Miss Porada.
[74] I, 13.
[75] G. S. Kirk, *Ships on Geometric Vases*, B.S.A. 1949.
[76] Reproduced in *Torr*, pl. 3.

170 being in three banks and thirty on the deck. Roman shipping extinguished the types of Phoenician craft previously known, and a Roman sarcophagus from Sidon in the 2nd century A.D.[77] shows a representation of a typical trading ship in which virtually the complete elimination of all earlier features derived from the Phoenician world has taken place.

A word remains to be said about the methods of navigation used in the pre-Greek world. Here the progress achieved seems to have been by the Phoenicians, whose efficiency and orderliness is praised by Xenophon.[78] Previous to them we gather that the sailor, out of sight of land, made use of birds such as the long-sighted crow, which he released from a cage and followed its flight to make landfall. This method is that apparently depicted on a Babylonian cylinder seal,[79] and is, in fact, that followed by Ut-Napishtim, the Babylonian Noah, in the Flood Legend, and his Biblical successor. In the Buddhist Jataka stories of the early years of this era, Babylonian merchants, travelling home by sea from India, use sighting crows. But such a method is obviously limited. For one thing, you cannot carry unlimited supplies of crows. Though Odysseus is said to have steered by the stars when he left Calypso, it is the Phoenicians who were credited with the true practice of that art. Greek writers, such as Strabo and Aratus, speak of the Phoenicians as their masters in maritime affairs,[80] and in particular, as teaching them the better way to distinguish the North by means of the Lesser Bear, in place of by the Great Wain or Plough. By this method, they could, by sailing at night, outstrip others who hugged the coasts. The Phoenicians possessed some charts and records of winds and distances, and the detailed record of Hanno's journey round West Africa was set up publicly in a temple. Ezekiel[81] draws, in poetic language, the picture of the world-wide network of Tyrian trade and alludes to Tyrian pilots. Tyrian pilots already led Solomon's and Hiram's fleets in the 10th century to Ophir, though according to others, the best pilots were from Sidon. Palinurus, it is claimed, the pilot of Aeneas, may have been a Phoenician. The site of Ophir has, it is true, not yet been identified in India, but the recent find of a potsherd at Tel Qassile in Palestine inscribed '*gold from Ophir for Bethôrôn*' shows that the Biblical references to Ophir and its gold are no mere myth.[82] The Phoenicians were clearly interested in the Eastern trade. In 590 B.C., the Egyptians started cutting a canal from the Nile to the Red Sea. It was left to Darius to complete it, marking its course along the isthmus of Suez by nine cuneiform inscriptions. Of this enterprise, the Phoenicians were probably the beneficiaries. In an article[83] in this Journal some years ago, Dr Harden pleaded for investigators to seek the sites of Phoenician trading stations in West Africa. They might likewise do well to look for that of Ophir in India.

[77] Dussaud *et al.*, *La Syrie Antique et mediévale illustrée*, pl. 33.
[78] *Oeconomica*, VIII.
[79] Frankfort, *Cylinder-Seals*, pl. XI, *m*. I owe this observation to Professor D. D. Kosambi.
[80] E. R. Taylor, *The Haven-Finding Art* (1956).
[81] ch. XXXVI.
[82] For references to Ophir, possibly Suppara, near Bombay, see my *Catalogue of the Nimrud Ivories*, 59, 60, 168.
[83] ANTIQUITY, September 1948.

ANTIQUITY XXXII, 1958

Rouffignac

by D. A. E. GARROD

Dr Dorothy Garrod reviews, in this article, the following three books: Rouffignac ou La Guerre des Mammouths, *by Louis-René Nougier and Romain Robert* (*Editions de la Table Ronde, Paris,* 1957*; pp.* 311, 33 *plates,* 1200 *francs*)*;* The Cave of Rouffignac, *by Louis-René Nougier and Romain Robert* (*translated by David Scott. George Newnes, London,* 1958*; pp.* 230, 39 *plates,* 36*s.*)*; and* Les Cavernes Touristiques du Périgord *by Bernard Pierret* (*Imprimerie Périgourdine, Périgueux,* 1957*; pp.* 130, 12 *plates,* 600 *francs*).

THOSE who were present at the closing session of the Congrès Préhistorique de France at Poitiers on 20 July, 1956, will remember the announcement by Professor Nougier of the discovery in the Dordogne of a new painted cave, whose name and exact location were not on that occasion disclosed. The manner of the announcement, and the elaborately organized publicity with which it was launched, made an unfavourable impression on many auditors, but this unease gave way to astonishment six days later, when it became known that the cave in question was one which had been well known and visited since the 16th century, had been intensively explored by speleologists, beginning with Martel, and had been visited on separate occasions by Denis Peyrony and the Abbé Breuil. All this, though disquieting, was not in itself sufficient to discredit the discovery. The cave of Miremont, or Rouffignac, as it has now been baptized, is vast, one of the largest in France, with a network of ramifying galleries more than seven kilometres in length. Before the works recently carried out by the owner, M. Charles Plassard, the first engravings, which are 600 metres from the entrance, could be reached only after very heavy going through intensely sticky clay. Peyrony, who was always on the lookout for new cave paintings, told his son that there was nothing at Miremont, but we do not know how far he penetrated. Breuil, who made a short visit in 1915 with the entomologist Charles Alluaud, was looking for a particular species of *Trechus*, and did not go far from the entrance. There was, therefore, a perfectly reasonable explanation for the failure of two leading experts to discover the paintings and engravings, and the Abbé himself, when he visited the cave with the discoverers three days before the announcement at Poitiers, did not hesitate to pronounce in favour of their authenticity.

The battle began with the entry into the arena of the speleologists. The members of the Speleo-Club of Périgueux had explored the cave in the course of forty visits between 1945 and 1949, spending over a thousand hours underground. They made a complete plan, which differed from that made by Martel in 1893 chiefly in the mapping of a system of lower galleries with active streams, not explored by him. A young schoolmaster named Bernard Pierret, who had been the leader in this work, affirmed in July 1956, and has repeated since, that in December 1948 he and his companions saw the frieze of three rhinoceroses outlined in black, which is one of the more remarkable features of Rouffignac, on a wall two metres from the place where they habitually set up camp inside the cave—a wall which up till then had been blank. It is certain that in July 1949 Pierret showed these

paintings to M. Severin Blanc, Inspector of Prehistoric Antiquities for the region, who declared them to be fakes. Other statements made by the young speleologists were rather contradictory, and it was not difficult for Professor Nougier to hold them up to scorn, but on this point of the non-existence of the rhinoceros frieze before December 1948 they remained firm, and they were upheld by an older colleague, M. William Martin, a former President of the Speleo-Club, who had taken part in the explorations up to July 1948. As against this, it should be recorded that M. de Laurière, owner of a house facing the cave on the far side of the valley, declared in August 1956 that he had visited Miremont twice in the summer of 1938, and had seen the paintings, but was deterred from mentioning them by the memory of Glozel. On being taken over the cave in September 1956 he specified that he had seen the three rhinoceroses, some mammoths and a horse.

On 12 September, 1956 a number of archaeologists who had been invited by Professor Nougier and M. Robert, met at Rouffignac for a ' visite de contrôle scientifique.' (The present writer was not able to accept, and visited Rouffignac for the first time in August 1958.) When the inspection was over, two documents were presented for signature. One, drawn up by Professors Almagro and Graziosi, upheld without reservation the authenticity and exceptional beauty of the paintings and engravings, the other, drafted by Professors Grassé and Leroi-Gourhan, while finding no evidence of non-authenticity, witheld final judgment pending the results of certain scientific tests. The first was signed by nearly all the visitors, the second by a small minority. There were only three abstentions; Professor Balout wished to avoid the appearance of checking the opinion of Breuil, while Mlle de St Mathurin and Dr Glyn Daniel, who had received an unfavourable impression from the visit, left the cave before the gates closed on the deliberations of their colleagues, because they preferred not to sign anything at all. (In *La Guerre des Mammouths* this incident is somewhat watered down, in the following sentence: ' De très rares collègues s'abstinrent, par principe en quelque sorte, ne jugeant pas cette déclaration opportune, tels Mlle de Saint-Mathurin et le Professeur Balout ' (p. 252)).

During the winter of 1956–7 a running battle continued, but by the spring the sceptics had been more or less worn down by the heavy artillery which was brought up against them, not least by that very big gun, the Abbé Breuil. Yet doubts persisted, and the publication of *Rouffignac ou la Guerre des Mammouths* did nothing to allay them.

It must be said at once that this is a bad book. As an account of the discovery it is tendentious and unscientific, as a pamphlet it is bombastic, pretentious and lacking in wit and style. Of the general tone and attitude, the least said the better. In the English translation the worst personal attacks have been cut out, but enough remains to give an unpleasant taste. The book is divided into three sections, dealing with the Prehistory, the History and the Discovery of Rouffignac, but the sections overflow into each other, as there is a great deal of repetition and anticipation. The Prehistory of Rouffignac, in the style of a popular manual, contains evocations of life in the Vézère valley during the Ice Age, alternating with descriptions of the paintings, and of their prototypes, as found refrigerated in the Siberian tundra. The History of Rouffignac deals with various texts relating to the cave from 1575 onwards, which have come to light since the discovery. The purpose is to show that the paintings had already been seen by a number of visitors in the 16th, 17th and 18th centuries. This thesis has impressed many people, but Mlle de St Mathurin has pointed out to me that the ' mosaics ', ' altars ' and ' paintings ', which each author borrows from the last, are part of the stock description of caves at that time, and that in any case the word ' peintures ' would never in the 16th and 17th centuries have been applied to the black outlines of the paintings at Rouffignac.

To come to the 19th century, Martel, on his plan of 1893 uses the word ' foirail ' (market

place) to designate a point where three galleries meet, opposite the rhinoceros frieze. The silence of Martel, who fully explored the cave and described it in his book *Les Abîmes* (1894), but did not mention the presence of paintings, is a fact which calls for explanation. Professor Nougier is aware of this, and has read into the word ' foirail ' a proof that Martel did in fact see the paintings, making an enigmatic note to that effect on his plan, but keeping silence in view of the scepticism aroused by the discovery of Altamira in 1879. As against this, Mlle de St Mathurin draws my attention to Martel's later book, *La France Ignorée*, published in 1930, when Palaeolithic art was a well established fact. In this he describes the paintings and engravings of La Mouthe, Les Combarelles, Pech-Merle, Altamira, and reverts to Rouffignac in the following words, ' Bref, tout l'intérêt du Cro de Granville est exclusivement d'ordre géologique '. In view of this, it is difficult to believe that the word ' foirail ' on his plan of thirty-seven years before was an aide-mémoire to remind him of the position of the rhinoceroses. If so, it did not fulfil its purpose.

The Discovery of Rouffignac relates the finding of the paintings and engravings by the authors, the visit of the Abbé Breuil, the announcement to the Poitiers congress, and the polemic which followed. It is here that the Périgueux speleologists, to whom the authors invariably refer as ' les petits spéléologues ' or ' les gamins ', get their worst trouncing. That Professor Nougier was irritated by Bernard Pierret and his companions is understandable; that he should lose his temper in print, in such a big way, months after the event, is more difficult to excuse.

The English translation of the book conveys quite adequately the style and tone of the original, but it contains a number of mistakes, some of which could have been avoided by the use of a Petit Larousse, others by consulting a prehistorian. For example, throughout, the translator describes the flints which are embedded in the limestone of the cave as flint ' kidneys ', but in the geological, as opposed to the anatomical sense, ' rognons ' are lumps, and ' rognons de silex ' are flint nodules. Again, ' plans de frappe à facettes du Levalloisien VII ' becomes ' facet stampings of the Seventh Levalloisien '. Other mistakes, due to an insufficient knowledge of French idiom, occasionally alter the sense of the text.

Bernard Pierret's book has no pretension to be anything but a guide for tourists, containing short descriptions of the principal caves and rock-shelters of the Périgord. Its main interest lies in an appendix, ' Précisions sur Miremont '. This is Pierret's answer to *La Guerre des Mammouths*. It is a pity that he has been tempted, in certain passages, into paying Professor Nougier back in his own coin—not very successfully, which is perhaps to his credit. In the first section of *La Guerre des Mammouths*, Nougier indulges in some rather heavy fun on the subject of the scratches made by bears on the cave walls, pretending to have counted 13,524,978 such scratches, and envisaging detailed statistical studies. M. Pierret takes this very seriously, and has no difficulty in showing it to be ridiculous. He does not perceive that the whole thing is meant to be a satire on the cumulative graphs of Palaeolithic industries beloved of M. Bordes and his disciples. These notes do, however, make some small but useful contributions to the Rouffignac controversy. Pierret, speaking also for his companions, reaffirms that they saw the rhinoceros frieze for the first time in December 1948 and, since it was on the wall immediately behind their camp, they considered it impossible that they should have missed it if it had been there at an earlier date. Pierret also corrects certain statements in *La Guerre des Mammouths*. On p. 208–9 (p. 151–2 of the English version) Professor Nougier gives a somewhat imaginative account of the visit paid to the cave by M. Severin Blanc with Bernard Pierret in July 1949. In spite of M. Blanc's own statement that he saw the rhinoceros frieze on that occasion, Professor Nougier concludes, in capital letters, that he never went so far, because if he had, it was inconceivable that he should not also have seen the frieze of eleven mammoths close by.

Pierret now repeats unequivocally that Blanc did go as far as the rhinoceroses, and saw them, and declared them to be fakes.

Finally, Pierret clears the name of a fellow speleologist unjustly held up to shame. The middle rhinoceros of the three has the inscription ' Dubois ' scratched in large capitals on its side. Although Dubois as a common surname is in the same class as Jones or Robinson, Professor Nougier, giving, for reasons which are not clear, the date 1950 for the inscription, at once assumed that this was done by M. Dubois, a speleologist from Brive, who visited Rouffignac in 1955 with his group. ' Pour sa gloire et sa honte, le rhinocéros portera donc son nom pour toute l'éternité. On passe à la postérité comme on peut ' (p. 222). Pierret now explains that his friend J. Dubois never signed his name on a rhinoceros, still less in 1950, when he had not yet visited the cave.

If one blows away the temperamental smoke which has obscured the issues at Rouffignac, it emerges fairly clearly that the doubters have, all the same, a case which cannot be met by mere invective. Obviously, neither Martel nor Peyrony could be called to testify to what they did or did not see in their day, but there were other witnesses who deserved to be heard with more patience and attention than they received. The young speleologists became confused, and began to contradict each other under the questioning of journalists and the onslaught of professors. Professor Nougier has a passage on this which is perhaps more revealing than he intends. ' Mais les spéléologues étaient jeunes, et ils étaient plusieurs. Et ils devaient peu à peu " se couper ", s'infliger mutuellement des démentis, perdre pied, manquer d'audace et de persévérance dans l'erreur ' (p. 211). Now that the battle has died down, the whole dossier of Rouffignac deserves to be re-examined calmly and objectively. That the immense prestige and authority of the Abbé Breuil, placed without reserve on the side of authenticity, should have caused the scale containing the speleologists to kick the beam is not surprising. One can only say, with great respect for the weight of his opinion, that those of us who know ourselves to be lesser fry by comparison are not thereby absolved or debarred from trying to form our own judgment in this puzzling case.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. *The listing of a book in this chronicle does not preclude its review in* ANTIQUITY.

THE EARLY SCULPTURE OF ELY CATHEDRAL. *By* GEORGE ZARNECKI. *pp.* 52 *and* 101 *photographs. London: Alec Tiranti,* 1958. 18*s.* A volume in the Tiranti *World of Beauty* series.

THE ANCIENT AND HISTORIC MONUMENTS OF THE ISLE OF MAN. *pp.* 48. *Douglas: The Manx Museum and National Trust,* 1958. 1*s.* 6*d.* A brief general guide, well-illustrated with a selected list of the more typical and best-preserved monuments to be visited.

A MANUAL FOR NEANDERTHALS. *By* H. MEWHINNEY. *Austin: the University of Texas Press,* 1957. $3.50. The author describes his experiments in flint knapping and how he was stung into making these experiments by reading M. C. Burkitt's views that making flint tools was difficult. ' It is easy to copy Aurignacian work', writes Mewhinney, ' also easy to excel it. To imitate the best Solutrean work takes more skill.' A challenge to modern knappers and forgers.

[*continued on p.* 267

The Excavation of the West Kennet Long Barrow: 1955-6

by STUART PIGGOTT

The Chambered Long Barrow at West Kennet in Wiltshire, is one of the most famous megalithic tombs in the British Isles. It was first excavated by John Thurnam in 1860, and recently re-excavated, with remarkable and surprising results, by Professor Stuart Piggott and his staff and students of the Department of Prehistoric Archaeology in the University of Edinburgh. Here Professor Piggott summarizes the results of his excavations in 1955 and 1956: the full report, now in preparation, will be published as a monograph of the Research Committee of the Society of Antiquaries of London.

THE West Kennet Long Barrow, lying on a ridge of chalk downland some two miles south of Avebury in North Wiltshire, is one of the most famous monuments of its type.[1] If one excludes the Dorset ' bank-barrows ', it is one of the longest, measuring some 330 ft. long on its east-west axis, 70 ft. wide at the eastern end, and 40 ft. at the western. Remains of the stone-built burial chambers and façade at the east end have been visible since it was first recorded by John Aubrey in the 1660's; William Stukeley made drawings in 1723 and 1724 which show the stones in the same order (or confusion) as in 1955, and in 1860 John Thurnam excavated beneath the westernmost of these, revealing a burial chamber with approach passage, only part of which he was able to dig owing to fallen stones he was not allowed to move. In the chamber Thurnam found a primary deposit of skeletons on the floor, beneath a filling which contained numerous sherds and flints, as well as animal bones, the pottery belonging almost wholly to Secondary Neolithic types of Peterborough and allied fabrics, with a certain amount of Beaker. His and W. C. Lukis's plans of the site showed a passage and terminal chamber, and left the ' very operose congeries of huge stones ' (Stukeley's phrase) otherwise uninterpreted.

The published plan presented an awkward puzzle to the typologists of chambered tombs, and the relationship of the surprisingly abundant pottery in the chamber filling to the interments was unresolved. The barrow had been one of the earliest Ancient Monuments to be scheduled by Pitt-Rivers, as the first Inspector under the Act of 1882, and after the last war the then Chief Inspector of Ancient Monuments, the late Mr Bryan O'Neil, discussed the site with the writer and invited him to re-excavate the barrow on behalf of the Inspectorate, with a view to elucidating, if possible, its archaeological peculiarities, and preparing the way for its conservation as a monument under guardianship for the benefit of the public. Excavations were accordingly carried out by the Department of Prehistoric Archaeology in the University of Edinburgh under the direction of the writer and Professor R. J. C. Atkinson in 1955 and 1956, with very interesting results.

Briefly, the main features were as follows. The structural aspects of these are graphically expressed in the accompanying isometric diagram (FIG. 1), and the typological problem was immediately resolved by the wholly unexpected and entirely gratifying discovery at a relative early stage of the excavation that Thurnam's excavations missed four intact burial chambers which, with the terminal chamber he did excavate, combine to make a plan for

[1] G. E. Daniel, *Prehistoric Chamber Tombs of England and Wales* (1950), 227, with refs.

which many parallels exist among the Severn-Cotswold group of chambered tombs. The passage from which these chambers open leads into a shallow semi-circular forecourt, distinctively not of Severn-Cotswold type, however, which after the final funerals had been elaborately blocked with boulders and a ' false entrance ', and closed by an enormous stone flanked by two somewhat smaller blocking stones, so set as to line up with a straight façade running across the east end of the barrow. The mound was found to have a core of sarsen boulders capped by chalk rubble derived from flanking quarry-ditches, but no trace of the peristalith suggested by Aubrey's sketch could be found. In the intact chambers the funeral ritual could be seen to have been that common to many chambered tombs, the bones of the earlier occupants being placed on one side to admit the later burials, but a wholly unexpected feature was the subsequent filling of all chambers and the passage right up to the roof with chalk rubble and occupational debris brought from elsewhere, and containing large quantities of pottery, bone and stone tools, beads and other objects. With the primary burials, and in direct association with the actual structure of the monument, was found pottery of Windmill Hill Neolithic type, and the sherds from the subsequent filling comprised a wide variety of Secondary Neolithic wares, including Peterborough and Rinyo-Clacton types, as well as various forms of Bell-Beaker pottery.

We may now consider the various features in greater detail. It had been the original intention to cut a section across the full width of the mound at some distance back from the terminal chamber, and to excavate in search of the flanking ditches whose existence was demonstrated by an electrical resistivity survey. The ditch on the north was in fact sectioned, and found to be nearly 10 ft. deep with an irregularly quarried base, and in its weathered condition, some 20 ft. wide at the top. It was separated from the mound by a berm nearly 40 ft. wide. A partial section only was made of the mound, since it was found that virtually the whole of the south side had been disturbed in recent times, consisting of craters some still visible on the surface and more filled up. This mutilation must be that referred to by Stukeley, who recorded that ' Dr Took has miserably defaced South Long Barrow by digging half the length of it ': ' Dr Took ' must be the Dr Toope of Marlborough who was in correspondence with John Aubrey in 1685, and whose depredations are referred to again below. At its maximum unmutilated height the mound of the barrow rose 14 ft. above a thick chocolate-coloured old surface line, its core, up to 6 ft. in height, consisting of large sarsen boulders with air-spaces between, capped by chalk rubble derived from the flanking quarry-ditches for the remainder of its height. On the old ground surface was a scatter of small sherds of Windmill Hill ware, undecorated and with a few simple rims represented.

When excavation began in the area of the known chamber and passage at the eastern end of the mound, practically the only stones which could be assigned to a function were those standing or leaning in an approximately straight line as a façade to the barrow. Outside these lay an enormous recumbent stone which we now know to have been the central blocking-stone, fallen outwards, and westwards of the façade, piled on the eastern slope of the barrow, lay a meaningless jumble of blocks, two of which were in fact the undisturbed cap-stones of the two northern chambers, and a third the displaced cap of the south-east chamber. As the removal of obviously non-structural stones, and the excavation of the area proceeded, it became clear that considerable wreckage had taken place over what was finally discovered to be the blocked forecourt and the passage immediately westwards of it; the cap-stone of the south-east chamber, as we have seen, had in fact been dragged off southwards. As Stukeley's drawings show us that the site was already in this state by 1723 (the south-east cap-stone was certainly as we found it in 1955, and the other stones, less surely identifiable, seem also to be in the same state), the wrecking must have taken place

before that date. The most likely suspect is Dr Toope, of whose activities Stukeley was told, and who certainly dug in what may have been a Saxon cemetery near The Sanctuary for human bones to make ' a noble medicine that relieved many of my distressed neighbours '. If he had hoped for further bones at West Kennet, the peculiar circumstances of the deliberate filling of the burial chambers in antiquity prevented him; the disturbance over the eastern chambers extends to a depth of 3 ft. only, leaving 5 ft. of undisturbed filling before the primary burials on the chamber floors could be reached.

The plan of the burial chambers on complete excavation showed a symmetrical composition, with a wide passage some 23 ft. long leading to a polygonal western or terminal chamber, and with two pairs of lateral chambers opening from it, three of these being approximately rectangular in plan, while one (the north-west) is polygonal. The proportions are massive: the terminal chamber is 9 ft. square and the others proportionately large. The construction is of blocks of local sarsen joined by panels of dry-stone walling, which is in some instances of sarsen boulders, but mainly of oolite of a type best represented by beds between Frome and Bradford-on-Avon, some twenty-five miles south-west from West Kennet.[2] The cap-stones are carried on two or more courses of sarsen corbels above the uprights at a height of 8 ft. above the floor in the two eastern chambers and in the terminal (western) chamber, and 6 ft. in the south-west and north-west chambers. These two chambers had their entrances blocked by small sarsens just over 3 ft. high set on the old surface and not in stone-holes as are all the structural uprights (PLATE XXV, *a*).

The passage opens on to a shallow semicircular forecourt 30 ft. across and about 7 ft. deep, which at its extremities joins to an approximately straight façade 20 ft. long on north and south, forecourt and façade being constructed of sarsen uprights joined by dry-stone walling in the same manner as the passage and chambers. The core of sarsen boulders already described as forming the spine of the mound continued round the chambers, and seems to have been held by revetment walls immediately behind the forecourt, this and the façade being backed by chalk rubble.

The central area of the façade at its junction with the passage had been badly wrecked by the probably 17th century digging already mentioned: no cap-stones of the passage at this point remained *in situ*. It could be established, however, that two massive upright stones had stood in the forecourt, more or less continuing the line of the sides of the passage, and on each side of these the forecourt was filled with a blocking of sarsen boulders. Whether these uprights had carried a lintel could not be decided owing to the destruction at this point. Immediately in front of the uprights three large stones had been set so as to continue the line of the façade as a chord of the forecourt arc. The central and southern of these stones has fallen outwards and lay flat, the northern was leaning outwards and had been partly destroyed. The central stone measured 13 by 10 ft. and when re-erected in its original stone-hole rested against the two uprights within the forecourt area. The whole of this construction must be interpreted as an exceptionally massive forecourt blocking set up at the final closing of the tomb.

An incidental discovery of some interest was that on more than one of the chamber uprights, and on the southern of the three blocking-stones, were polished areas where stone or flint axe-blades had been ground or resharpened (PLATE XXV, *b*). The construction of such a megalithic tomb as West Kennet must have involved the use of considerable quantities of timber for levers, rollers and similar purposes, with a corresponding necessity for axes for felling and trimming.

[2] Report by Professor W. J. Arkell, Sidgwick Museum, Cambridge. Thurnam records a whetstone from the secondary filling made of Pennant Sandstone from the valley of the Bristol Avon.

Sherds of Windmill Hill Neolithic pottery were found in the stone-hole of one of the façade uprights, and in that of one of the passage uprights as well. This, taken in conjunction with the sherds already noted as being present on the old surface beneath the mound, and those associated with the primary burials just to be described, enable us to assign the building and original use of the tomb to the earlier phases of the type-site itself, lying only a couple of miles away to the north. Sherds of Peterborough Ware from the forecourt blocking suggest that the work here, like the filling of the chambers, is to be associated with the use of this particular pottery style, though as we shall see this distinction may be cultural rather than rigidly chronological.

The floors of chambers and passage consisted of the same old ground surface as observed under the mound, and the burials had been laid direct on this. As usual in collective tomb burials there were, in addition to identifiably complete or partially complete skeletons, quantities of disordered bones. The remains were mainly concentrated at the rear of the chambers; in the north-east chamber a complete crouched burial of an elderly man occupied the western corner (PLATE XXV, *c*), with a leaf-shaped flint arrowhead in the region of his throat, and above him a mass of cremated bones representing two individuals, one male and one female. In the south-west chamber a row of three skulls had been laid against the rear wall, and a cache of vertebrae and long bones was similarly placed in the north-west chamber. Thurnam had found the remains of five individuals in the terminal chamber, and Professor Wells[3] assesses the material from the four chambers excavated in 1955 as representing upwards of twenty adults, one youth, and at least a dozen children and infants—the latter were particularly abundant in the south-east chamber. All Thurnam's burials were male, and of the remainder of the adults nine were male and the rest female.

Professor Wells was able to demonstrate some very curious features in the circumstances of the West Kennet burials. In the first place there are not enough skulls to go round, and more lower mandibles than skulls. This (as he says) suggests that skulls were removed after the remains were reduced to skeletal form, in some instances leaving the lower jaw behind, and still more odd is the fact that there is a similar shortage of the larger limb-bones, which may also have been removed from the tomb after burial. He stresses that the circumstances preclude an explanation in terms of the transference of the bones from a temporary mortuary to the present tomb, and in this connection one may note that a skull and several femora were found in the upper part of the forecourt blocking, as if dropped while in the process of removal. One femur Professor Wells would assign to a skeleton in the south-east chamber, and this is confirmed by the presence in the same area of the blocking of a large sherd of a very distinctive Peterborough pot, other fragments of which were in fact found in the same chamber.

In terms of physical characteristics the West Kennet burials show features which are consonant with regarding them as members of a related group or groups—three or four parents with well-defined characters could, Professor Wells thinks, account for all the variations in skull-form present. He also feels that the West Kennet material may represent a mingling of two types, one robust and the other more gracile, and that the cranial characteristics would support Morant's contention of a North European strain in the British Neolithic population.

In the south-east chamber, against the west wall, were the crushed remains of what had been a complete bowl of Windmill Hill ware, with a slightly thickened rim decorated with oblique lines of ' imitation cord ' pattern, and other sherds of unornamented pottery of undifferentiated Windmill Hill type were found on the floor of the north-east chamber.

[3] The human remains have been reported on by Professor L. H. Wells, formerly of the University of Edinburgh and now of the University of Cape Town.

It has already been mentioned that one of the skeletons in this chamber had a leaf-arrowhead associated with it. Otherwise, grave-goods in the normal sense were not present, but subsequent to the final deposition of burials (and after the presumed robbing of the tomb for skulls and long-bones) the chambers and passage were deliberately filled with irregular layers of chalk rubble, mainly clean but with many seams and patches (up to a foot or so in thickness) of rubble stained brown and black with charcoal dust and containing abundant potsherds, animal bones, flint flakes and occasional implements, bone points and other tools, and frequent beads made of bone, stone or perforated shell. A similar but less concentrated scatter occurred sporadically in the clean layers.

The sherds varied in size from small scraps to large fragments representing half a pot or more, and represented various forms of Peterborough Ware, Rinyo-Clacton Ware, and Bell-Beakers. No Windmill Hill ware was present, nor was there any sequential stratification among the various types of pottery, sherds of Beaker, for instance, occurring in the lowest layers (immediately above the primary burials) in the north-east chamber as well as at other levels. A striking find was the greater part of a fine Bell-Beaker carefully placed upside down in an angle of the north-west chamber at a high level.[4] In sorting the pottery it has been found that no whole pot is represented, that fragments of the same vessel may be scattered between two or more chambers, and that many pots survive only as a group of sherds none of which join. In other words, the pots were not broken in the tomb as a part of funerary ritual, but were brought there as already scattered potsherds. Although burnt fragments of bone were relatively common, and other signs of burning and fire, no fire had been made within the tomb at any stage of the filling.

The filling had been carried up completely to the underside of the capstones, and one can only assume that the far chambers were filled first, those responsible for the process backing out in what would seem to us a rather undignified manner. The filling of the five chambers and the passage would amount to some 2,500 cu. ft. of material. The impression given by the mixture of dirty rubble and broken artifacts was that in part at least soil and rubbish had been scraped up from the floors of settlement-sites, or perhaps from temporary camping-places connected in some way with the funeral ritual. The forecourt and the area in front of the façade were completely stripped, and showed no signs of any such occupation or use, so that an origin at some distance from the barrow seems likely. Such filling has hardly been recognized as a distinctive feature in chambered tombs elsewhere, but something similar is known in the British Isles as well as in Scandinavia and North Europe.

The pottery from the filling comprises an overwhelming percentage of Peterborough Ware, comparable with that found by Thurnam in the west chamber but amplifying to a marked degree our knowledge of the sub-varieties of this ceramic in southern England. The Ebbsfleet and Mortlake variants are present, as well as many examples of what Dr Isobel Smith has called Fengate Ware,[5] with small flat bases and overhanging rims foreshadowing the Cinerary Urns of which it is the progenitor. Rinyo-Clacton Ware is represented by a few finds only, and no vessel can be reconstructed, though they were clearly of the normal flat-based type. All the Beaker sherds found in 1955 are of the Bell-Beaker family (Abercromby's Type B1) and include not only the fine vessel already mentioned, but sherds of Cord-Zoned Beakers (Childe's B3).

The flint-work is uninformative, consisting mainly of waste flakes, with rounded or discoidal scrapers as the commonest tool-type, and one leaf-arrow in addition to that already mentioned as being found with one of the burials. In Thurnam's excavation a very

[4] Illustrated in J. F. S. Stone, *Wessex Before the Celts* (1958), pl. 14. Other illustrations of West Kennet are in pls. 6–8.

[5] In an unpublished thesis.

fine polished-edge knife of characteristic Secondary Neolithic type was found. A broken chalk mace-head with cylindrical drill-bored perforation has been reworked into a clumsy pendant, and the fairly numerous beads are of simple globular or barrel form in shale, chalk or bone, and are in some instances extremely small (e.g. bone beads of from 3 to 5 mm. in external diameter). Perforated shells include the little tusk-shaped *Dentalium*. Bone points or awls are relatively abundant; there are a number of perforated phalanges and a couple of ' scoops ' or polishers.

All the foregoing finds occurred in the filling of the chambers and of the passage where this remained intact; for half its length this, and the western chamber, had been dug out by Thurnam and refilled in modern times. Except for one or two Peterborough sherds and a human skull and other bones already mentioned, the forecourt blocking contained no finds, nor was there any trace of ritual features or finds in the 20-ft. wide excavation made in front of the whole length of the entrance and façade.

The results of the new excavations at the West Kennet Long Barrow have (as so often is the case) cleared up certain difficulties but raised many new problems. The recovery of the plan of the burial chambers shows that far from dealing with an abnormality we have in fact the finest and largest example so far known of a tomb-type well known in the Severn-Cotswold group of chambered cairns, and hitherto best represented by Notgrove and Uley in a form most nearly comparable to West Kennet, and with other characteristic variations on the same basic plan in Stoney Littleton, Nympsfield or Parc le Breos Cwm: Wayland's Smithy, referred to again below, is another simplified version of the same ' transepted ' plan. A remarkable feature at West Kennet is the comparatively great height of the passage and chambers—6 to 8 ft. Whether we call such tombs passage-graves or gallery-graves seems a semantic irrelevance, but the fact which is of importance is that comparable tomb-plans do occur in a restricted area of Western France around the mouth of the River Loire. Since G. E. Daniel first demonstrated the architectural relationships between the plans of his Retz Group and those of the Severn-Cotswold tombs,[6] the position of the former as ancestral to the English series has been generally accepted. Before the new West Kennet excavations, the date of the Cotswold tombs could not be fixed more precisely than within the general span of the Windmill Hill culture in southern England, but we now know that a great transepted tomb of this group was being built two miles from the type-site of the culture, and at a stage early in the stratified sequence of the ditches of the causewayed camp there. The implications of this for the chronology of British chambered tombs is of course hardly less significant than for that of the West French sites.

Although the plan of the West Kennet burial chambers is recognizably a version of the Severn-Cotswold formula, the semi-circular forecourt opening on to a straight façade is without parallel in the southern British tombs, though a regular feature in the Clyde-Carlingford series of tombs such as East Bennan or Carn Ban in Arran.[7] It is possible that Wayland's Smithy had a similar forecourt (still unexcavated) with fallen blocking stones, and Professor Atkinson's excavation of the Manton Long Barrow, four miles north-east of West Kennet, in 1955, revealed a plan which suggests an asymmetric degeneration of the same arrangement. Stukeley's drawings of the Devil's Den and of the destroyed chambered tomb on Temple Down again suggest the presence in the Avebury district of tombs with façades more comparable in plan to the Clyde-Carlingford series than to the Severn-Cotswold,[8] and strengthen one's belief in some fairly close connection in architectural

[6] G. E. Daniel, *Proc. Prehist. Soc.*, v (1939), 143.

[7] S. Piggott, *Neo. Cultures Brit. Isles* (1954), fig. 25: contrast the Cotswold plans in fig. 20.

[8] *Wilts. Arch. Mag.*, LII (1947), 57, where I suggested that such tombs might ' stand nearer to the main stream of ideas in tomb architecture than the rather specialized forms of the Cotswolds '. West Kennet now provides welcome confirmation of this view.

traditions between these two groups, with the cusp-shaped forecourts of the Cotswolds as perhaps a later local development in that region.

The placing of the burial chambers at West Kennet at one end of an enormously long mound, part sarsen cairn and part rubble thrown up from flanking quarry-ditches, again emphasizes the complexities of tradition combined in our chambered long barrows and cairns. Here one has surely to look to the unchambered long barrows whose origins may well lie not in the west of Europe, but rather eastwards of the British Isles in the North European plain. The Windmill Hill culture, which not so long ago looked so deceptively homogeneous, may well turn out to be a complex affair with multiple origins in many European Neolithic traditions.

This complexity of coexisting traditions in material culture is again implied by the mixture of pottery in the chamber and passage infilling. The recent excavations at Windmill Hill* have shown that the apparent simplicity of the stratified sequence in the ditches there was illusory, and that sherds representing both the Abingdon and the Ebbsfleet pottery styles do in fact occur sporadically as a component of the pottery content in the lowest levels, though this is dominated by the plain wares named from the type-site. With this in mind, the presence of sherds of pottery of at least three Late Neolithic cultures—Peterborough Ware (with its variants), Rinyo-Clacton Ware and Bell-Beakers—in the filling of the chambers seems to represent cultural rather than chronological differentiation, for there is no evidence that any long interval elapsed between the last burials and bone-robbing, and the filling-up of the tomb. Indeed, the evidence of the forecourt blocking, while not conclusive, does suggest that this final act of closing a collective tomb built by users of Windmill Hill pottery was associated with people using Peterborough Ware: a state of affairs comparable with that observed at Notgrove and Nympsfield as well as in the Clyde-Carlingford tomb of Cairnholy I in Galloway.[9]

On the other hand, the fact that the only pottery associated with the original building of the tomb, and of its primary use, was of Windmill Hill types, and that none of this ware occurred among the secondary chamber filling, does mark some sort of a distinction between the makers of pottery in this tradition and those responsible for the mixture of Secondary Neolithic fabrics, exclusively in secondary contexts, on the same site. We must regard the West Kennet Long Barrow as a monument of the Windmill Hill culture, and one constructed and first used at a relatively early phase of the use of the causewayed camp at the type-site so short a distance away.

The phenomenon of the deliberate secondary filling of the chambers and passage seems hardly to have been recognized as a feature in the chambered tombs of the British Isles. On the other hand, it must be remembered that many of our better-known tombs have been, at the time of excavation, long roofless and ruined; both Nympsfield and Notgrove are examples of this state of affairs, and chamber filling could have existed. Nor can we be sure that the filling present in less ruined sites need always be the result of the later collapse of walling, etc., and, indeed, such sites as Pipton, where Dr Savory found that one chamber had in all probability been deliberately filled with clean earth, and another probably filled in a similar manner, shows a comparable situation, though in this case without the inclusion of artifacts.[10] At Ty-isaf, too, Professor Grimes found the chambers full of stone debris, perhaps as a result of later ruin, perhaps not.[11] Similar circumstances

* (Of which a preliminary note appears on p. 268.)

[9] E. M. Clifford, *Arch.*, LXXXVI (1937), 119; *ibid.*, *Proc. Prehist. Soc.*, IV (1938), 188; S. Piggott and T. G. E. Powell, *Proc. Soc. Ant. Soct.*, LXXXIII (1948–9), 103.

[10] H. N. Savory, *Arch. Camb.*, 1956, 7.

[11] W. F. Grimes, *Proc. Prehist. Soc.*, V (1939), 119.

may have obtained in some Clyde-Carlingford tombs (e.g. at Clontygora Large Cairn; Carn Ban, Arran),[12] and at Brackley, Kintyre, the deliberate filling up of the chamber as part of the final blocking (and after an Early Bronze Age burial with a jet space-plate necklace had been deposited) was noted.[13] In fact, the deliberate total or partial filling of a collective burial chamber after the point of its final use may not be so unusual as at first appears. It was clearly not a universal practice, in the British Isles or elsewhere, as the unfilled chambers at Lanhill or other sites show, but, nevertheless, it does recur both here and on the Continent. Dr Lili Kaelas has pointed out to me that sporadic instances of such deliberate filling are known from South Scandinavian and North German chambered tombs, and others suspected from excavation reports which did not recognize the phenomenon for what it was.[14] Similarly in the British Isles we must be prepared to accept deliberate filling where the evidence demands it, and not necessarily explain away the stone and earth contents of chambers as the result of accidental penetration of silt or of later ruin of walling or roof.

The large quantities of artifacts in the filling of the West Kennet chambers considerably augment our rather imperfect knowledge of the material culture other than pottery in the Secondary Neolithic groups. The pottery itself includes as we have seen a notable series of fragments of vessels of Fengate Ware which go far to provide links between this pottery style and that of our Cinerary Urns and Food Vessels, although at the same time others extend the range of eccentricity within the Secondary Neolithic pottery repertoire and show unparalleled forms.

The new excavations at West Kennet have then advanced our knowledge not only of this particular site, but also of many aspects of the British Neolithic cultures. In particular, the phenomenon of deliberate in-filling in collective chambered tombs needs investigation, both here and on the Continent, and the forecourt plan at West Kennet raises once more the question of the affinities which may exist between the architectural traditions of the Severn-Cotswold tombs and those of the Clyde-Carlingford group. And apart from these and other results of concern to the professional archaeologist, the work of conservation and judicious restoration which the Ancient Monuments Department of the Ministry of Works has carried out since the excavations has provided the general public with an opportunity of seeing one of the most magnificent and impressive of our collective chambered tombs. To see the monumental façade with its huge blocking stones at the entrance, and to walk into the great burial chambers, is an experience which brings home to one the architectural capabilities of those builders in massive stones who, before 2000 B.C., were already in North Wiltshire mastering the basic techniques which were to be used by their successors at Avebury and at Stonehenge.[15]

[12] O. Davies and T. G. F. Paterson, *Proc. Belfast Nat. Hist. and Phil. Soc.*, 2nd S., I, pt. ii (1936–7), 20. T. H. Bryce, *Proc. Soc. Ant. Scot.*, XXXVII (1902–3), 36.

[13] J. G. Scott, *Proc. Soc. Ant. Scot.*, LXXXIX (1955–6), 22.

[14] I am indebted to Dr Kaelas for allowing me to see the proofs of a paper to be published in *Offa* where this subject is discussed.

[15] The finds from the 1955–6 excavations at West Kennet will be deposited in the Museum of the Wiltshire Archaeological and Natural History Society at Devizes; some restored pottery is at present on exhibition in the City Museum, Birmingham.

The 'Ancient Indian Style' Seals from Bahrain

by GEOFFREY BIBBY

In 1953 *a Danish Archaeological Expedition was sent out by the Prehistoric Museum of Aarhus to work on the island of Bahrain in the Persian Gulf. It continued its work in subsequent years and since* 1957 *has extended its interests to other areas in the Gulf, to the peninsula of Qatar and to Kuwait. The expedition has been directed by Professor Glob of Aarhus; for detailed reports of its work readers are referred to the annual reports in* Kuml, *the Journal of the Jutland Archaeological Society. A general account of the results of the first four years was published in* The Illustrated London News *for* 4 *and* 11 *January*, 1958. *Here Geoffrey Bibby, who is on the staff of the Aarhus Museum, discusses the problems of the Bahrain seals: and Colonel Gordon and Sir Mortimer Wheeler comment on his views.*

AMONG the more significant of the objects discovered by the expedition in Bahrain are a number of circular stamp-seals. These seals are of the type to which attention was first drawn by Professor C. J. Gadd in a famous article in 1932.[1] In this article Gadd discussed and illustrated eighteen Mesopotamian seals, of which sixteen were found at Ur, all clearly bearing evidence of a connection with the civilization of the Indus Valley. One of these was a square seal of typical Indus Valley character, bearing, like many of them, a figure of a bull but, in the place of the normal inscription in Indus characters, a cuneiform inscription. Two of the others were cylinder seals bearing bull-figures reminiscent of the Indus stamp-seals. The remaining fifteen seals were circular, with pierced bosses on the back, the bosses ornamented with from one to three grooves and, in at least seven cases, with four centre-pointed circles. Gadd recorded in addition, but did not illustrate, two further seals of Mesopotamian origin of the same type. Their connection with the Indus valley is shown clearly, partly in that three similar seals have been found at Mohenjo-daro, and partly in that six of the seals bear a figure of a bull surmounted by an inscription in the Indus Valley script, while two further seals (nos. 15 and 18) bear signs, above figures of bulls, which it is possible to interpret as more naturalistically drawn hieroglyphs, some of which can be paralleled in the Indus script. The remainder of the seals bear more varied designs, some of which are quite definitely Sumerian in conception.

We have thus a uniform type of seal, found both in Mesopotamia and in the Indus Valley (though 'foreign' in both environments) and bearing variously typical Indus and typical Sumerian designs, as well as some which appear to partake of both natures. It is therefore extremely interesting to find these seals occurring on Bahrain, a site which lies close to any conceivable sea route between the two major civilizations, especially as so far this is the only type of seal to have been found on Bahrain. It is difficult to avoid the hypothesis that these seals are in fact 'native' to Bahrain, and that where they occur in Mesopotamia or the Indus Valley they have been introduced by Bahraini travellers or resident merchants.

Five seals have so far been found in Bahrain, and two fragments of seals of the same type on the island of Failaka in the principality of Kuwait. The fifth Bahrain seal and the Kuwait

[1] See Gadd, 'Seals of Ancient Indian Style found at Ur', *Proceedings of the British Academy*, XVIII.

fragments were found this year and are not yet accessible for description and illustration. Seal V from Bahrain, however, resembles seal II closely in the reverse, and on the obverse bears a picture of two opposed warriors armed with spear and shield.

Seal I[2] was discovered in 1953 by an American engineer on the surface of the desert not far from a group of burial mounds and is now in private hands in America. It is 2·6 cm. in diameter, of grey-white steatite. The reverse bears a shallow boss with three grooves and four centre-pointed circles. The obverse has a picture of a bull, surmounted by another quadruped with a head resembling that of a cock, and, above, three smaller figures, including a scorpion (PLATE XXVI, *a*, *b*).[3]

Seal II was found in 1957 in a stratified occupation level in the city-site of Qala'at al-Bahrain. It is 2·6 cm. in diameter and 1·1 cm. high, of grey steatite with a white ' glaze '. The reverse bears a shallow boss with three grooves and four centre-pointed circles. The obverse has four figures, unrelated to each other in so far as they have each a different ' up ' direction. They comprise a chequerboard, a human figure with outstretched arms and legs, a standing human figure, and a bull which appears to be hung by its horns with trailing legs (PLATE XXVI, *c*, *d*, *e*).

Seal III was found in 1957 in an area which had been disturbed by early Islamic stone-quarrying close within the city-wall at Qala'at al-Bahrain. It is 2·8 cm. in diameter and 1·6 cm. high, of grey steatite with a white ' glaze '. The reverse bears a high boss with a single groove and no circles. The obverse has the figure of a bull surmounted by those of a scorpion and a human footprint (PLATE XXVII, *f*, *g*, *h*).

Seal IV, from a stratified occupation level in Qala'at al-Bahrain, about a metre deeper than seal II, was found in 1957 and is considerably smaller than the others, measuring 2·0 cm. in diameter and 1·5 cm. in height. It is of black steatite, without ' glaze '. The reverse bears a high boss with two grooves and no circles. The obverse has a figure of a quadruped, more resembling a goat than a bull, with, above its back, a starlike figure and, viewed from the left-hand side, a gazelle (PLATE XXVII, *j*, *k*, *l*).[4]

Seal V, not here illustrated, was found in 1958 at the temple-site of Barbar, in an area which had been disturbed by early Islamic stone-quarriers.

Since Gadd's article in 1932 it has been realized that the seals of the type here described are important evidence for the dating of the Indus Valley civilization. Unfortunately, only three of the Ur specimens were found in contexts susceptible even of rough dating, coming respectively from a Sargonid grave (Gadd no. 15), a Second Dynasty of Ur tomb-shaft (Gadd no. 16) and a Kassite (?) rubbish level (Gadd no. 12). As will have been noted, two of the Bahrain seals, nos II and IV, are from undisturbed levels and are therefore potentially dateable, at least to the extent of a *terminus ante quem*. The whole sequence of Bahrain dating is, however, in the absence of inscriptions, subject to considerations of parallels of pottery and artifacts with Mesopotamia, and can only with caution be brought into relation with Sumerian and Babylonian chronology. There are, however, good grounds for attributing the levels in which seals II and IV were found to the Sargonid Period, or even to the preceding Early Dynastic III.

Col. D. H. Gordon comments as follows:

There appears to be some danger of misapprehension concerning certain seals recently discovered in excavations by the Danish archaeological expedition at the city site of Qala'at

[2] See Mallowan, ' The Indus Civilization: a Review '. ANTIQUITY, December, 1955, p. 200.
[3] This seal is published in *Kuml*, 1954 (Universitetsforlaget, Aarhus).
[4] Seals II, III and IV, and the whole chronological question, are discussed in *Kuml*, 1957.

al-Bahrain. Close similarities with certain seals discovered at Ur undoubtedly exist, but it is the expression ' of ancient Indian style ', originated by C. J. Gadd, which requires much closer examination. Good clear photographs of four seals, three of which have already been published in the *Illustrated London News*,[5] have been available for study. These fall into three different types, two of which were found at Ur and not one at any Indus Valley site nor, for that matter, anywhere else in India.

It is this final statement which calls for a reassessment of the seals published by Gadd under the title of Seals of Ancient Indian style found at Ur.[6] It must be emphasized that of the eighteen seals examined by him, five only, 2 to 5 inclusive and 16, have inscriptions which are clearly in Harappan characters and a high narrow boss with a single incised line, putting them in the same category as the three circular seals of the same type found at Mohenjo-daro,[7] which are clearly a scarce and abnormal type as distinct from well over a thousand square seals as commonly found at Harappan sites. There are also a square seal (1) with an ox and a line of cuneiform characters which apart from its general arrangement, and even in this the animal faces the wrong way, has no similarity with the Indus seals; a circular seal of uncertain pedigree (18) which has an inscription in what might be a clumsy rendering of the Harappan script and two cylinder seals (6 and 7), one of which has an Indian humped ox and the other the Harappan motifs of a ' unicorn ' with a second head rising from its back and a fish sign, both suggestive of Indian contacts. These seals, having varying degrees of affinity with the Harappa culture, I will call Type I. Then there is seal 15 from Ur showing a bull similar to those on the circular seals with Harappan script. But only one character of the inscription can be paralleled in that script, that which may be taken as representing a man; all the rest are pictographs of naturalistic style. This seal, very vaguely connected with India, I call Type II.

Apart from these, it is most important to note that there is a class of seven seals, 8 to 14 inclusive, which have no resemblance to Indus seals whatsoever. Furthermore not a single seal of this type has ever been found anywhere in India. They are circular and have a broad boss covering almost the whole of the back of the seal, on which are three parallel lines across the centre, save on one which is so small as only to allow of one line, and have two dotted circles in each of the divisions so made; on the other hand, Indian seals are square and have a high narrow boss with a single line. Moreover, not one solitary one of this class of Ur seals has a design remotely resembling anything produced in the Indus valley, and are therefore not seals of ancient Indian style and have no connection with India at all. True they are made of steatite, but even this is not specifically Indian, as, along with lapis lazuli and shell, this is one of the commonest materials used for seal-making at Ur. Of these seals five were ' without context ', one was a surface find and one from upper rubbish, Kassite (?) level; so, though found at Ur, we know neither their date nor their place of origin. These I call Type III.

Of the seals from Bahrain there is not one seal of Type I showing clear parallels with the Harappa culture, nor is there one of any type that has been found in India. Bahrain seal III can be said to correspond to Type II above in that it has a bull and two naturalistic symbols, a foot-print and scorpion, on its face, and a bull and a scorpion feature on seal 15 from Ur. It has a high relatively narrow boss with no circles. Bahrain seals I, II and an unillustrated V have the same type of wide boss with three parallel lines and four dotted circles on the

[5] ' Excavating a Bahrain Citadel of 5000 years ago; and seal links with Ur and Mohenjo-daro ', by Geoffery Bibby, *Illustrated London News*, 11 January, 1958.

[6] ' Seals of Ancient Indian style found at Ur ', by C. J. Gadd, *Proc. of the British Academy*, vol. XVIII, 1932.

[7] *Mohenjo-daro and the Indus Civilization*, Sir John Marshall and others, pls. CX, 309 and CXIV, 478; *Further Excavations at Mohenjo-daro*, by E. Mackay, pl. XCVI, 500.

reverse as Ur Type III, and their obverses bear nothing that resembles anything that is found in India. Bahrain seal IV is unlike anything yet found either in India or Iraq.

The above are all statements of fact, but some opinions may be ventured. The problem of Bahrain is a very interesting and important one, and it is possible that these seals may help to solve it. Some day such seals may come to light in India, but so far they have not; Bahrain may have been Dilmun and it was almost certainly an entrepôt on the trade route to India, and so it is possible that seals of this kind were carried on to the Indus or to ports in Kathiawad and will some day be found in those localities, though this will not necessarily make them Indian or even of Indian style. All this is conjecture and dates are very insecure; a possible late Sargonid grave for Type II and upper rubbish, questionably Kassite, for one of Type III are unstable chronological foundations. That Bahrain as a port of call for ships sailing the Persian Gulf originated these seals seems doubtful, that they can be used to date all the 100,000 burials on Bahrain to the 3rd millennium B.C. as suggested in the *Illustrated London News* is more than doubtful. Objects already recovered by three earlier excavators indicate 2nd and even early 1st millennium B.C. datings for a number of these tombs.

Sir Mortimer Wheeler adds:

My friend Colonel Gordon does not perhaps give Professor Gadd sufficient credit for realizing and stating as early as 1932 that the Mesopotamian seals, which in his classic paper he associated with the Indus Valley, ' differ from the great majority of those found in India. . . . The import into Babylonia probably did not come from Mohenjo-daro or Harappa. But since it is evident that many other sites of the same civilization must have existed, and will doubtless in time be explored, this conclusion is of no great interest.'

Twenty-six years later this cautious judgment remains valid; outside the metropolitan sites and one or two others, our knowledge of the Indus Civilization is still very ill-balanced. Discoveries in 1957 showed that the Civilization extended at least as far southwards as the estuaries of the Narbada and the Tapti, giving an Indus coastline (to date) of 800 miles. Apart from some useful excavation at Lothal in Kathiawad, scarcely anything is known in detail of this significant coastal spread, and what surprises may await us when the Indus ports are adequately explored are anybody's guess.

But as things stand at present the main facts about the seals are clear enough. Except for the half-dozen steatite seals from Sumer bearing unequivocal Indus script, the ' Gadd ' seals and now those from Bahrain show only a vague affinity with those available from the Indus. But that vague affinity is not negligible. The button form with loop-handle, whether grooved and diversified by dotted circles or not, is a true if tenuous link between southern Mesopotamia, Bahrain and the Indus. I have the authority of Professor Mallowan for affirming that such seals are entirely absent from the northern half of the Tigris-Euphrates valley, and they certainly did not enter Babylonia from Phoenicia, Syria or Asia Minor. They came, therefore, either overland through Persia or from the Persian Gulf—or, more probably, as their diversity suggests, were made at various entrepôts (such as Bahrain itself) of a cosmopolitan Persian Gulf trade of the kind which has been analysed by A. L. Oppenheim from Larsa tablets (*Journal of the American Oriental Society*, 74 (1954), 6–17).

For the present, therefore, I propose the over-all name ' Persian Gulf seals ' for this mixed bag. Their date is uncertain but 2000 B.C.± is probably a safe estimate.

The Hamburg Conference, 1958

by T. G. E. POWELL

The Fifth International Congress of Prehistoric and Protohistoric Sciences was held in Hamburg this year from 24 to 30 August. The previous conferences in this series had been held in London (1932), *Oslo* (1936), *Zurich* (1950), *and Madrid* (1954). *The Zurich conference was described for readers by Professor Piggott* (ANTIQUITY, 1950, 171). *In this article Mr Powell describes the Congress in general, and the Excursions* (*Excursions II and III*) *which took place after. Dr Charles McBurney adds a note on the work of the Congress in Palaeolithic and Mesolithic studies.*

AN international congress of some 700 members, faced with a programme of over 250 communications, divided into ten sections, and in full session for five and a half days, is an occasion beyond comprehensive and equitable summary by one pen. In these pages, an attempt is made only to record something of the character of the Congress as a whole, together with its excursions, and, at the same time, to offer some more personal comments arising from observations that the writer was able to make during the lecture periods in Hamburg, and thereafter on Excursions II and III.

Following that memorable Fourth Congress in Madrid in 1954, the Fifth Congress took place within the walls of the University of Hamburg, and under the presidency of Professor Dr Gerhard Bersu, who has recently retired from the Directorship of the Römisch-Germanische Kommission, Frankfurt-am-Main. A word must be said in praise of the arrangements in the University, and the suitability of choice for a congress of this size and kind. Each section had its own large lecture-room with proper projection facilities. The great entrance hall provided a much needed informal gathering place, as did the adjoining canteen, and an outstanding attraction, within the entrance hall, was the truly extraordinary exhibition of archaeological books and periodicals, of over a thousand items, which had been organized by Dr Rudolf Habelt of Bonn. This, and a lavish supply of publishers' leaflets, provided a unique opportunity for congress members to survey the great range of international archaeological literature that has appeared since 1954. Opportunity should also be taken here to acknowledge the splendid hospitality of the Federal Government's reception on board the motor-ship *Jan Molsen* during a cruise on the Elbe, and that of the Senate of the Free and Hanseatic City of Hamburg in the Rathaus. For the undoubted success of the Congress, the thanks of members have gone unhesitatingly to Professor Bersu, and the German organization committee, as also to the directors of museums and excavations who made special preparations, and to the large number of younger persons who were on duty in various capacities in Hamburg and during the excursions.

The supply of Congress literature was such that it was delivered to each member in a large paper carrier. Apart from a competent Official Programme, with full lists of members under different classifications, list of communications, and time-tables, two publications of outstanding merit were also provided. In the first place, comes the massive volume *Neue Ausgrabungen in Deutschland*, edited by Dr Werner Krämer for the Römisch-Germanische Kommission (xii + 604 pp.; numerous plates, text figs., and folders. Verlag

Gebr. Mann, Berlin, 1958). This volume contains forty-four contributions describing important excavations and discoveries since the Second World War. There are contributors from East Germany, and a large section of the book is devoted to excavations at sites of the Migration and Medieval periods. It will be perhaps wisest here to avoid mention of particular excavations to the exclusion of others, but this is an essential work of reference, and the contributions are fully supported with bibliographical notes.

The second publication of note supplied to Congress members is a double number of the East German periodical *Ausgrabungen und Funde* (Band 3, Heft 4/5, 1958. Institut für Vor-und Frühgeschichte, German Academy of Sciences in Berlin). Here, the editor is Professor Wilhelm Unverzagt, and the numerous contributors describe not only excavations, but give important summaries of the present state of knowledge on particular chronological and cultural problems. Again, excavation of Medieval sites is seen to form an important aspect of current archaeological activity. The publication ends with some pages of information on the organization of archaeology in East Germany, and a list of the archaeological periodicals now published at Berlin, and at the various provincial centres.

A small book offered to members of the Fifth International Congress by Polish archaeologists was also issued. This is *Archäologische Forschungen in Polen* by Witold Hensel and Aleksander Gieysztor (74 pp., 45 figs., 1 folder. Polonia Verlag, Warsaw, 1958). This contains a summary of archaeological progress over the whole field, together with a list of archaeological organizations and their publications. There is a select bibliography of fifty-four books, and other publications, issued between 1945–57 on Polish archaeology.

If any comment can be made to apply to the content of the lecture sessions in Hamburg as a whole, it is one of disappointment at the incapability of so many contributors to present a concise statement of essential and relevant information within a time-limit of which the duration was known long before they need have felt obliged to offer a communication. There can be really no explanation, other than intellectual incapacity, for speakers to exceed their time allocation at an international congress of this order. This is supposed to be a gathering of scholars, and to fail to keep within the reasonable limits laid down by the Permanent Council, is an admission of incompetence no less than of discourtesy to the audience, who have engagements elsewhere, and to following speakers who, in many cases, suffered severe curtailment of their own time. Sectional presidents were not sufficiently severe in maintaining their schedules. It is, admittedly, a very difficult situation to handle, and perhaps only the ringing of automatic bells will enforce the necessary discipline until standards improve. It was clear, too, that many speakers had failed to give sufficient thought to the preparation of their communications. The worth-while essentials were often buried beneath a mountain of detail that was only appropriate to an appendix in some eventual publication.

In schools of archaeology in Britain and Ireland these matters engage close attention, and the outcome was apparent in the great majority of communications from those quarters. In the quality and selection of illustrations for lantern projection, speaking again generally, insufficient care was often apparent. Colour is excellent in a film, as it was, for example in Professor O'Kelly's demonstration of iron-smelting processes but, with what seemed to the writer at Hamburg rare exceptions, it proved itself no substitute for good black and white pictures for the clear appreciation of archaeological objects and sites, although this may have been due to insufficient care in the preparation and projection of colour slides.

One further criticism must be levelled. When time for discussion was available, there was all too great a tendency in some sections to avoid the wider issues of pan-European relationships, and to fasten instead on points of pedantry to do with almost local matters of terminology and chronology. This narrowness of outlook was noticeable in particular

on certain occasions in the Bronze Age section when a wide exchange of views would have been particularly useful. Despite these harsh words, there were a number of communications of the highest standard, both in content and presentation.

From amongst many informative papers a small selection of subjects and names is offered, with the reminder that a volume containing the texts of all communications delivered at the Congress will be published in due course. The most important general topic discussed was undoubtedly that of Carbon 14 chronology. This subject is dealt with elsewhere in this number of ANTIQUITY (p. 253), but at Hamburg some ten papers in Section I*b*, related directly to this dating technique, and contributors included such names as those of Prof. De Vries of Groningen, Dr Tauber of Copenhagen, Prof. Schwabedissen of Köln, and Prof. Braidwood of Chicago. It would appear that the disparities, and unduly high datings, that continue to perplex users of this technique stem rather from the difficulties of obtaining satisfactory material in archaeological deposits than from any inadequacy in the technique itself and its underlying propositions.

In the Neolithic Section, Prof. Mikov of Sofia gave an account of the stratigraphy of a tell at Karanovo, in southern Bulgaria, that he has excavated, and which gives important correlations between the Balkans and Troy. Danubian I houses excavated in Holland (by Prof. Waterbolk at Geelen, and Mr Modderman at Sittard) have shown two phases with different door arrangements in the middle of one of the longer sides of each house. The Funnel-Beaker Culture (TBC) and its connections with the Michelsberg Culture provided a subject of considerable interest, not least to British listeners, since the antecedents of the Windmill Hill Culture become more patently involved. On megalithic tombs in Atlantic Europe, there was some welcome information particularly from France, and this included Dr Giot's description of tholoi in Brittany, Monsieur Claude Burnez on long barrows, and also the ditched enclosures of Peu Richard type in the Saintonge, and Dr Glyn Daniel on the dating value of the hitherto much neglected *mobilier* from French megalithic tombs.

In the Bronze Age section, for brilliance of exposition, and certainly for interest to British archaeology, were the two papers by Miss Margaret Smith and Professor Hawkes. The first dealt with the results of spectrometric analysis of the metal in southern British Middle and Late Bronze Age hoards revealing fundamental differences between the old native, and new intrusive, schools of metallurgy. The second gave a new appreciation of the cultural zonation and chronology of the same two periods with an important redefinition of Late Bronze Age II. Dr Amelia Mozsolics' paper on the Hungarian Bronze Age was not given the discussion it so fully deserved; the time being frittered away on narrower issues closer to the *Nordischer Kreis*. Dr Marija Gimbutas surveyed the Bronze Age cultures of Eastern Europe, and proposed a regional nomenclature. In the face of some criticism for the names she used, it may be observed that no modern geographical nomenclature can ever be completely satisfactory for prehistoric cultures, or be devoid of some kind of historical implication. But, then, our Wessex culture causes no confusion although we all know that Wessex in an historical context has a very definite connotation. Dr Rolf Hachmann's consideration of the Khorsabad king-lists, Assyrian archaeology, and the absolute chronology of the European Bronze Age, gave expression to a new and wider avenue that is now being explored in Germany as in England.

In the Iron Age section, Dr Kromer's paper on the actual cemetery at Hallstatt brought welcome news that a modern study of the site and finds was at last being undertaken. Monsieur Raymond Lantier demonstrated how, through the large scale levelling operations for agriculture during the Middle Ages and since, the prehistoric land surface in large tracts of France may be as much as twenty or more feet below the present. A paper marking a real advance in Iron Age studies in Ireland was that by Mr Étienne Rynne who pro-

pounded a western and earlier, and a northern and later, province of La Tène art in that island.

Of later periods, and the two sections concerned were well attended and of great significance in the Congress, mention here will be made only of Prof. Behm-Blancke's paper on cult sites and bog offerings of the Teutonic peoples in Central Germany, Mr J. R. C. Hamilton's lucid description of the Scottish brochs, and Dr Ejnar Dyggve's reconsideration of the Slavic temple at Arkona in the light of its description by Saxo Grammaticus, and the excavations by Schuchhardt.

Field archaeology, and the inspection of provincial museums, plays as important a part as any other activity in achieving the purposes of the Congress. During the week in Hamburg, a whole-day excursion was made to the neighbourhood of Bremerhaven where the important excavations at Feddersen Wierde, under the direction of Dr Werner Haarnagel, have revealed timber built houses in a ' wurt ' settlement dating from about the time of Christ to the 5th century A.D. Before the formal opening of the Congress in Hamburg, Excursion I had started from Stuttgart, having first had a day in which to visit the splendours of the newly opened Landesmuseum. Amongst the more important sites visited in the first three days were the Heuneburg, the now famous late Hallstatt fortification on the Upper Danube, the oppidum at Donnersberg, possibly a stronghold of the Mediomatrici, situated west of the Rhine at the highest point of the Haardt range, and, nearer Frankfurt, the great hill-top stone-built fortification on the Altkönig, and the Roman establishment at the Saalburg. The excursion then split into two sections, the one devoted mainly to monuments of Roman times and the Teutonic expansion, the other to the megalithic monuments of north-western Germany. The two sections then reunited at Lüneburg before proceeding to Hamburg.

Excursion II took place immediately after the termination of the Congress at Hamburg, and consisted of three strenuous days in Schleswig-Holstein under command of Professor Sprockhoff. He was ably assisted by Professor Kersten, and Dr Hingst, and the very thorough and lucid way in which they explained the monuments visited, and the general nature of the terrain, will not easily be forgotten by those fortunate enough to attend this excursion. Most impressive of the antiquities visited were perhaps the immense long barrows at Putlos situated on downland overlooking the Baltic. These are single, not collective grave, monuments. Dr Bantlemann's excavations in a ' warft ' at Elisenhof near Tönning were visited, as also the excavations at Old Lübeck. The Landesmuseum at Schleswig which contains important collections of all periods was visited. Here is the Nydam boat, and many other peat finds of the first significance.

Excursion III followed the second, and started for East Germany from Lübeck. The number of places available had been limited to one hundred, but the actual figure of those attending was about eighty, and four skilfully driven motor coaches from Berlin conveyed the party throughout the whole extensive tour. It should be said that this Excursion had been planned, and paid for, on a purely professional basis, that this atmosphere was maintained, and that only strictly archaeological matters enter therefore into this account. The thanks of all participating members were fully expressed to the organizers of this excursion for a remarkable feat of efficiency, and forethought for the details of administration. Especially was this due to Dr H. Schubart, and Frau Wroblewski, and those in the predominantly English speaking bus have reason particularly to thank Frau Eva Kaufmann who stayed the complete course acting as interpreter and guide.

The Third Excursion extended over a period of eight days, and gave opportunity not only to see museums, but also a great deal of the topography of some of the most important archaeological regions in middle Europe. In the first three days the setting was the Baltic

coast, and the great moraine country of the north German plain. At Schwerin, the first stopping place, the former grand ducal museum was of especial interest not only for its collections but for its place in the history of archaeology, having, amongst other things, been the first in Germany to be arranged on the Three Age system. The older collections are not at present available at Schwerin, but amongst recent acquisitions should be mentioned the perforated red deer frontals and other mesolithic material from Hohen Viecheln. The museum at Stralsund contains a good regional collection of all periods, the principal area of prehistoric settlement having been on Rügen and throughout a belt of country running south-eastwards. A day was spent on Rügen, a delightful island with stretches of chalk cliff and large beech woods. Amongst monuments inspected was the great trapezoid long tumulus with massive stone setting at Nadelitz, and the great cliff-top earthwork of the Slavic stronghold at Arkona. On the following day, the excavation, at Behren-Lübchin, of an 11th–12th-century timber-built stronghold, now under peat, was visited on the way south-westwards to the second area of study. The rich collections in the Altmärkisches Museum at Stendal were a preparation for the enormous wealth of material, especially for Neolithic and Bronze Age times, that was to be found in the Landesmuseum at Halle/Saale. The whole area west of the middle Elbe, and around its great tributary the Saale, was of course one of the nodal points of activity and interchange in prehistoric Europe. In the Landesmuseum, where every possible facility for study was at the disposal of the visitors, the great diversity of Neolithic ceramic groups, the contents of Aunjetitz graves such as Leubingen, and hoards such as those from Dieskau, and the decorated stones from the Göhlitzsch and Dölauer Heide cists, were but the most outstanding of this museums' admirably displayed possessions. Halle was undoubtedly the high-light of the excursion for many of the participants, and all had reason for particular thanks to Dr Hermann Behrens and his staff for the nature of their welcome, and for the most timely refreshment which greeted the excursionists on their arrival after a long bus ride. At Dresden, the Landesmuseum is being set up in the Japanese Palace now under restoration, and here, and locally in the field, Dr Werner Coblenz was an indefatigable guide. Amongst sites visited in the region of Meissen was the excavation of a 10th–11th-century fortification, at Zehren, with timber and earth built rampart.

The final days included a visit to the collections of the institute for prehistory in the University of Jena, under the guidance of Prof. Neumann, and of the museum at Weimar under Dr Behm-Blancke. These both contain important collections, and at Weimar may perhaps be specially mentioned the rich chieftains' graves of the Roman Iron Age including that from Hassleben. A most remarkable *tour de force* in modern museum display has been accomplished at this museum.

There seems to be a great reluctance in German museums, as often elsewhere, to provide labels with the names of find-places. This is a great embarrassment to the time-pressed student, and must surely deprive the museum of much of that appeal to local interest which, in Britain, is known to be such a fruitful source for further acquisitions.

The last hours of the Third Excursion were spent in Berlin at a reception given by the Academy of Sciences. This lavish and admirably served supper will long be remembered as one of the greatest of archaeological parties.

Dr McBurney writes:

The highlights among the contributions to this Conference, in the Palaeolithic and Mesolithic fields, were, with one notable exception, more concerned with chronology and method than with individual discoveries. Above all, J. Evernden's paper on the newly perfected method of radio-active dating based on volcanic products gives great hope of

supplying the long-awaited absolute dating of the Lower and early Middle Palaeolithic. The few dates available so far, though no more than a beginning in themselves, hold out promise not merely of dating individual sites, but still more important of calibrating once and for all the vital sequence of temperature changes established by Emiliani on deep sea cores, and hence a large part of the ice age as a whole. An important paper bearing on the same subject with the same wide implications, but limited to the Last Glacial Maximum and the end of the Last Interglacial was read by Professor De Vries of Groningen, and supplied a whole series of dates back to 50,000 with specific probable errors. On the strength of these De Vries advanced the provocative claim that they support the existing calibration of Emiliani's curve, based on a revised reading of Milankovitch. Of greater immediate application were those of his dates which apply to the beginning of the Upper Palaeolithic of Europe, including one from a sample obtained by Mlle Henri Martin at La Quina for the Middle Aurignacian, of 35,000 (no error given), which compares with two of 32,000 for the same culture in Central Europe. Two papers on a statistical method of analysis of the Upper Palaeolithic and Mesolithic by A. Bohmers were of considerable interest as providing the first fully worked out system of the kind, to be applied on a large scale, as an alternative to that of F. Bordes. It was generally agreed that they had certain advantages over the latter though they left some members of the Conference a little stunned by their complexity, and the magnitude of the labour already devoted to them.

Beyond question the most important field result presented was that obtained by the joint labours of Professors D. A. E. Garrod and F. E. Zeuner on a new site at Adlun in the Lebanon proving in convincing fashion the contemporaneity of the Pre-Aurignacian of the Levant with a storm beach of late Last Interglacial date. It would seem that the protagonists of an ultra late dating for the Jabrud and Mount Carmel successions will find this one a hard nut to crack. It is likely that these results alone would render this conference a memorable one to all concerned with the earlier periods of prehistory, apart from the many other excellent contributions.

PAST AND PRESENT

No. 14. November 1958

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Radio Carbon Dating: Its Scope and Limitations

by HAROLD BARKER

In the last number of ANTIQUITY (1958, 193) reference was made to the criticisms *of Carbon* 14 *dating published by Milojčić, Elsasser and others. In this article Mr Barker of the Department of Research Laboratory, the British Museum, answers these criticisms, restates in simple terms the principles underlying the technique, and helps to bridge the gap between the archaeologist and the physicist. Milojčić's critical article* (Germania, 1957, 102) *has also been answered by Schwabedissen and Münnich in an article published* (Germania, 1958, 133) *as Mr Barker's article was going to press.*

THE technique of radiocarbon dating has been in use for nearly ten years and during this time several thousand dates have been published, a very large proportion of which have been accepted as accurate. The method is world-wide in scope, is applicable to a range of commonly occurring materials, and provides for the first time a system of chronology which is independent of hypothetical archaeological and geological considerations. During this period there has been steady progress both in the instrumentation of the method and in our understanding of certain underlying factors which now permit of a truer assessment of its capabilities. Furthermore, it is clear that the assimilation of statistical concepts—which may be unfamiliar to many users—is necessary in order to avoid pitfalls in the interpretation of the dates. A survey of the method would therefore appear desirable at the present juncture, particularly since serious doubts seem to have been raised in the minds of many archaeologists as to the ultimate validity of the method.

These doubts would appear to stem from two main causes, a long and detailed criticism by Milojčić,[1] and an article by Elsasser, Ney and Winckler[2] concerning the implication of certain magnetic measurements on the accuracy of radiocarbon dates. This latter article relates to specific work in the field of geomagnetism and will be briefly dealt with first.

According to the authors, the magnetic measurements made by O. and E. Thellier[3] can be interpreted as giving evidence of a marked change in the earth's magnetic field in the past 2000 years. Since the earth's field acts to some extent as a screen to cosmic rays which are in turn responsible for the production of all naturally-occurring radiocarbon, this would imply changes in the level of radiocarbon production in the past and might, therefore, affect the validity of carbon dating. On this basis it is calculated that a 2000 year old sample should be found to be 240 years too old. In order to check this hypothesis, Ostlund[4] made a series of very careful measurements on Sequoia tree rings of about this age. They were supplied to him by R. Sievert of the Radiophysical Institute at Stockholm, who did not disclose their true ages until Ostlund had completed his measurements. It was then found that the radiocarbon and true ages were in very close agreement so that this experiment provided no support for the magnetic theory. It must also be pointed out that the relevant magnetic measurements are concerned with only a relatively small area of the world so that extrapolation to a world wide effect may not be justified. Further work is obviously neces-

[1] Milojčić, V., *Germania*, 1957, 102–10.

[2] Elsasser, W., Ney, E. P., and Winckler, J. R., *Nature*, **178**, 1226 (1957).

[3] Thellier, E. and O., *C.R. Acad. Sci. Paris*, **212**, 281 (1941); **214**, 382 (1942); **222**, 905 (1946); **223**, 1476 (1951); **234**, 1464 (1952).

[4] Ostlund, H. G., *Science*, **126**, 493–7 (1957).

sary, but the present judgment must be that the effect is not proven and, even if it exists, is unlikely to influence greatly the accuracy of existing published dates.

Milojčić's article serves largely to illustrate the apparent wide gulf and total absence of common ground between archaeology on the one hand and the associated sciences of physics and chemistry on the other. In criticizing the basis of the method he admits that he approaches the matter from the point of view of a layman. Much of what he says serves only to demonstrate that he has either misread or misunderstood Libby's work concerning the establishment of the fundamentals of the method and has not grasped the statistical concepts which are necessary if one is to utilize the results properly. However, in view of the effects it may have produced on the minds of archaeologists one cannot dismiss the article as being totally unimportant. It clearly emphasizes the need for some restatement of the basic facts as well as the assumptions underlying radiocarbon dating, so that archaeologists may be able to distinguish between valid criticism and what Milojčić himself describes as the misunderstandings of a layman.

ISOTOPES AND RADIOACTIVITY

In dealing with radiocarbon dating we are also dealing with the properties of radioactive isotopes, a class of substances which have been extensively studied during the past twenty years. Isotope (ἴσος = equal ; τόπος = place) was the name suggested by Soddy in 1913 to denote elements which have different atomic weights but the same chemical properties and therefore occupy the same place in the periodic classification of the elements. Thus radiocarbon or carbon-14, with which we are concerned here, has an atomic weight of 14, whereas ordinary carbon (atomic weight = 12·011) consists of a mixture of about 99 per cent of the carbon-12 isotope (atomic weight 12) and 1 per cent of the carbon-13 isotope (atomic weight 13). All the carbon isotopes have the same chemical properties, but carbon-14 differs from the others in that it is also radioactive and exhibits the general properties common to all radioactive substances, i.e. its nucleus is not completely stable and tends to rearrange its internal structure with the emission of ' radiation '. In the particular case of carbon-14, this rearrangement involves transmutation of the carbon-14 atom into an atom of the nitrogen isotope of the same atomic weight (14) and the emission of a fast-moving electron known as a beta ray. This process of radioactive breakdown (or radioactive decay) follows laws which have been very well established. Thus the rate of decay (also known as the ' activity ') at any given time is proportional to the total number of atoms present at that time, and the time taken for a given fraction of the total number of atoms to decay is always the same.

THE HALF-LIFE

Different radioactive substances are characterized by their ' half-lives ', defined as the time required for any given quantity of the substance to dwindle to half that quantity by the decay process. Carbon-14 has a half-life of 5568 years and, like all other radioactive substances, its rate of breakdown is not influenced in any way by external conditions. This is an established fact of experimental physics. Thus we can be sure that the carbon-14 in a sample for dating will have decayed according to the established law independent of the conditions under which the sample has survived. Another of Milojčić's misunderstandings is that concerning the measurement of the half-life of carbon-14. This is derived from measurements which are completely independent of carbon-14 dating measurements, of the use of samples of known age, or of any knowledge of the absolute specific activity of living matter. One need not go into technical details as to how this is done, but it does seem necessary to emphasize this point in view of the fact that Milojčić not only states that the

half-life is dependent on an accurate knowledge of the specific activity of living material, but also implies that the results on historically dated samples were used by Libby to corroborate the value of the half-life. In point of fact, Libby was careful to point out that such data could not be used to decide definitely between the various experimentally measured values obtained by a number of workers.[5]

Experimental errors in the determination of the half-life naturally affect the dating results directly. A 1 per cent error in the half-life appears directly as a 1 per cent error in the absolute age of the specimen. Therefore, in assessing radiocarbon dates in terms of calendar years, the error in the half-life must be taken into account. However, in comparing carbon-14 dates with each other, errors in the half-life are of no consequence so long as the same value of the half-life has been used for each calculation of radiocarbon age. The value used by Libby (5568 $\pm$ 30 years) has been generally adopted by dating laboratories. If future measurements should indicate the need for a revision of this value, published results will have to be recalculated whenever they are to be compared with calendar dates.

THE ORIGIN OF CARBON-14

Carbon-14 originates from the action of neutrons of certain energies on the nitrogen isotope of atomic weight 14 (nitrogen-14). It was first discovered and characterized as an artificial man-made element, a product of atomic research and, as such, its properties and half-life were investigated long before carbon-14 dating was shown to be a possibility. Neutrons are known to be produced as secondary particles by the action of primary cosmic rays on the atmosphere at high altitudes, and the nitrogen-14 isotope is a major constituent of the atmosphere. It was from these considerations that Libby was led to predict that carbon-14 is continually being formed in the upper atmosphere as a result of processes identical to those which had already been studied in laboratory experiments.[6] Again, by invoking only the operation of known natural laws, it was predicted that this carbon-14 should be present everywhere in the world-wide carbon exchange reservoir which comprises the carbon dioxide dissolved in the oceans, the carbon dioxide in the atmosphere, all living plant and animal material (the ' Biosphere ') and also dissolved organic material in the oceans.

FUNDAMENTAL ASSUMPTIONS AND EXPERIMENTAL VERIFICATION

In order to apply carbon-14 dating, one must assume that the carbon-14 activity of this exchange reservoir has remained uniform and at the same level throughout the ages, and is therefore equal to the activity of the modern reference sample used by the dating laboratory. This was shown by Libby to follow from assumptions that the cosmic-ray intensity and the size of the exchange reservoir had both remained constant for many thousands of years. Practical proof that this assumption is correct, at least for the past 4000 years or so, was provided by the excellent agreement obtained by Libby between the carbon-14 ages and the known ages of historically dated materials. This agreement has been verified many times by other laboratories using more accurate methods. Evidence that the basic assumption also held for the period from 5,000–10,000 years ago, was indirect, and was based on the fact that the experimentally determined value for the absolute specific activity of carbon from the exchange reservoir was in reasonable agreement with that calculated on theoretical grounds. This was not in itself an accurate check, but it could be argued that if there had been big changes in cosmic ray intensity during this period, the agreement between the two values would not be nearly so good.

[5] Libby, W. F., *Radiocarbon Dating*, 2nd Edn., 1955, p. 42.
[6] Libby, W. F., *Phys. Rev.*, 69, 671 (1946).

One might mention at this point that the measurement of the absolute specific activity of modern carbon was only necessary for the purposes of this comparison between theory and practice. Ordinarily, carbon-14 dating work does not require its evaluation and this is fortunate in view of the difficulties in the way of a really accurate measurement. The fact needs stressing since Milojčić makes great play of the two values obtained by Libby (12·5 and 15·3 d.p.m. per gram) as representing a difference of 1300 years. Nothing could be further from the truth. In ordinary dating practice, one compares the activity of a certain quantity of carbon from the unknown sample with that of the same quantity from a ' modern ' reference sample, under exactly the same experimental conditions. In these circumstances the calculation of the age of the sample does not require any knowledge of the actual mass of carbon involved, nor does one need to take into account the fact that the counter detects only a proportion of the total number of carbon-14 disintegrations from each sample, for it is the same proportion in each case. On the other hand, in order to obtain the absolute specific activity of a sample, one must know exactly how much carbon is in the measuring equipment (the ' counter ') and also the efficiency of the counter for the detection of carbon-14 beta particles. This involves a long series of very careful measurements which are quite independent of carbon-14 dating. Thus Libby's early figure of 12·5 d.p.m./gm. was later changed to 15·3 d.p.m./gm. as a result of more accurate measurements of the efficiency of his counter. This change did not affect the dates in any way.

The results of the work on samples of known age, taken in conjunction with the agreement between theory and practice as regards the absolute specific activity of living carbon, gave grounds for reasonable confidence in the application of the method to the dating of suitable samples. Later work by Kulp and Volchok[7] provided more direct experimental evidence for the validity of the method. They dated samples taken from deep sea mud cores, both by the carbon-14 method, using carbon extracted from the precipitated carbonates, and by the ionium method, which is based on a measurement of the radium concentration in homogeneous cores. The results showed that any variations in cosmic ray flux during the past 35,000 years could not have exceeded 10–20 per cent. The uncertainties were due more to the experimental difficulties rather than to any definite fluctuations in the results.

STATISTICAL CONCEPTS

The accuracy of the basic assumptions of the method is thus confirmed at least to within the limits of the experimental errors involved. However, Milojčić finds Libby's results ' remarkably contradictory '. Clearly this is due to difficulties over statistical concepts, and if one is to use radiocarbon dates properly there is no alternative but to familiarize oneself with these.[8] It is hoped that what follows will help, if only by pointing out the basic difficulties and limitations which are inherent in the measurements.

In principle, carbon-14 dating is simple. One assumes that the carbon in the sample to be dated had, originally, the same carbon-14 activity as ' modern ' carbon. The activity of the sample is measured, as is that of a modern reference sample, and these measured values are used in conjunction with the half-life of carbon-14 to calculate the age of the sample as follows:

$$\text{Age} = \frac{2{\cdot}303}{0{\cdot}693} . T . \log \frac{A_0}{A_s}$$

where T = half-life of carbon-14.
A_0 = activity of ' modern ' reference sample.
A_s = activity of sample being dated.

[7] Kulp, J. L., and Volchok, H. C., *Phys. Rev.*, **90**, 713 (1953).

[8] An excellent and entertaining introduction to statistics is provided by *Facts and Figures*, by J. Mahoney (Pelican No. A 236: Penguin Books 1953).

PLATE XXI

(*a*)

(*b*)

(*c*)

EARLY SHIPPING IN THE NEAR EAST. (*a*) Assyrian *maqurru* from cylinder seals; (*b*) Clay model from Eridu; (*c*) Silver model of canoe from Ur.

Photos: (*b*) *Illustrated London News.* (*c*) British Museum.]

[*See p.* 221

(*a*) An Assyrian relief of the 7th century B.C.

See p. 228]

[*Photo:* British Museum.

EARLY SHIPPING IN THE NEAR EAST. (*b*) A sculpture from Nineveh: the flight of the Phoenician fleet, before Sennacherib's attack, in 700 B.C.

[*Photo:* British Museum.

EARLY SHIPPING IN THE NEAR EAST. A vase from Cyprus, showing a 'round' ship, freighted with wine.

[*Photo:* British Museum.

See p. 227]

ANTIQUITY was founded in 1927 by Dr O. G. S. Crawford

ADVISORY EDITORS

Professor Dr Gerhard Bersu, Dr G. H. S. Bushnell, Professor M. E. L. Mallowan, Professor Stuart Piggott, and Sir Mortimer Wheeler.

Printed by
The Whitefriars Press Ltd.
London and Tonbridge

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(*a*) Filling in N.W. chamber, seen from partly excavated passage.

(*b*) Axe sharpening marks on upright of S.W. chamber.

WEST KENNET LONG BARROW. (*c*) Articulated primary interment in angle of N.E. chamber.

(*a*)

(*b*)

(*c*)

(*d*)

(*e*)

Plates XXVI and XXVII

Seal I. (*a*) Obverse; (*b*) Reverse (Casts).

Seal II. (*c*, *d*) Obverse and Impression; (*e*) Reverse.

Seal III. (*e*, *f*) Obverse and Impression; (*g*) Reverse.

Seal IV. (*j*, *k*) Obverse and Impression; (*l*) Reverse.

SEALS FROM BAHRAIN

[*See p.* 244

PLATE XXVII

(*f*)

(*j*)

(*g*)

(*k*)

(*h*)

(*l*)

SEALS FROM BAHRAIN

[*See p.* 244

The immense practical difficulties in the way of these measurements and the methods by which they are overcome are matters outside the scope of this article, but it will be seen that errors in these measurements are bound to affect the accuracy of the calculated age. It is the special nature of these errors which impose very definite restrictions on the accuracy obtainable by the radiocarbon method.

Radioactive decay is not a smooth process, but is characterized by discrete events, each involving the disintegration of an atomic nucleus. Because of the random nature of the process, the decay rates or activities which one measures in are subject to short-term fluctuations. The problem of assessing the error introduced by this effect is rather like that involved in measuring, let us say, the average height of an Englishman. Strictly speaking, this involves measuring the heights of all Englishmen, a somewhat impractical solution to the problem. Time and labour can be saved by measuring only a fraction of the population and assuming that it is accurately representative of the whole. One might go into the street and select the first ten men one meets and measure their heights in order to arrive at an average. This procedure is open to the objection that the inclusion of an unusually tall man, or a dwarf, would influence the results unduly, for such people do not form anywhere near as much as one-tenth of the population. Common sense says that the average height of a group of one hundred men would be more likely to be close to the true average height of all Englishmen, and that of ten thousand might be very close indeed. Furthermore, one might expect the averages of many groups of ten men to differ a great deal amongst themselves, scattering very widely around the true value, whereas such deviations from the true value would be less for groups of one hundred and much less for groups of ten thousand.

This same sort of scatter is found to occur when repeated determinations of radioactivity are made on a given sample. If the separate observations are based on only a few atomic breakdowns the scatter is very wide, but grows less as larger numbers of such events are involved in each measurement. Thus it is possible to reduce the error due to such scatter to permissible proportions by basing the measurement on the disintegration of a sufficiently large number of atoms. Even so, the error can never be entirely eliminated.

In a large number of individual measurements the most frequently occurring values are those close to the true value, whilst less frequently there occur values which are much wider of the mark. These deviations obey definite statistical laws, and although it is not possible to state with absolute precision just how far any one estimation will differ from the true value, the lack of precision can be put into a useful quantitative form. Since in any group of such measurements the most frequently occurring values are those closest to the true value, it is much more probable that any single determination will be close to the true value than that it will be a widely different value. Errors in activities are therefore most usually expressed in terms of a quantity known as the ' standard deviation ', which takes this probability into account as it is fundamentally related to the law governing the manner in which these quantities scatter about the true value.

A concrete example may serve to illustrate these points. In table 1, column A contains the results of thirty separate measurements of the activity of the same sample (actually the ' modern ' reference sample used in the British Museum laboratory). The average of such a large group of measurements is sufficiently close to the true value for it to be regarded as such for the purposes of this calculation since the error of the average is so very much less than the errors of the individual measurements. Column B then gives the difference or ' deviation ' of each measurement from this average or ' true ' value. Column C gives the ' standard deviation ' of each measurement. This is calculated from the number of atomic breakdowns involved in each measurement, and turns out to be the same for each measure-

TABLE 1

A Activity	B Deviation from Mean	C Standard Deviation	A Activity	B Deviation from Mean	C Standard Deviation	A Activity	B Deviation from Mean	C Standard Deviation
15·7	+ 0·2	± 0·4	15·7	+ 0·2	± 0·4	14·8	− 0·7	± 0·4
15·5	0·0	± 0·4	15·8	+ 0·3	± 0·4	15·0	− 0·5	± 0·4
15·7	+ 0·2	± 0·4	15·5	0·0	± 0·4	16·4	+ 0·9	± 0·4
15·0	− 0·5	± 0·4	15·0	− 0·5	± 0·4	15·6	+ 0·1	± 0·4
15·7	+ 0·2	± 0·4	14·7	− 0·8	± 0·4	16·2	+ 0·7	± 0·4
15·3	− 0·2	± 0·4	16·1	+ 0·6	± 0·4	15·1	− 0·4	± 0·4
15·3	− 0·2	± 0·4	15·6	+ 0·1	± 0·4	15·7	+ 0·2	± 0·4
15·6	+ 0·1	± 0·4	15·4	− 0·1	± 0·4	15·7	+ 0·2	± 0·4
16·0	+ 0·5	± 0·4	15·7	+ 0·2	± 0·4	15·5	0·0	± 0·4
15·8	+ 0·3	± 0·4	15·2	− 0·3	± 0·4	15·2	− 0·3	± 0·4

COLUMN A lists the results of thirty separate measurements of the same activity. Each measurement is based on about 1500 disintegrations in a 100-minute ' counting ' period.

COLUMN B lists the deviations of the individual activities from the mean value of 15·5 disintegrations per minute (the average of the thirty measurements).

COLUMN C lists the standard deviation of each measurement. It is calculated from the number of disintegrations involved in the measurement, and is the same for each measurement because this number is approximately the same in each case.

ALL THE FIGURES ARE EXPRESSED AS DISINTEGRATIONS PER MINUTE (d.p.m.)

ment because very nearly the same number of such disintegrations have been involved each time.

It will be seen that in the case of two-thirds of the measurements (twenty out of thirty), the individual deviations from the ' true ' value are within the limits of one standard deviation from that value, and that twenty-nine out of thirty are within the limits of two standard deviations from the true value. If a very large number of such measurements are made it is found that 68 per cent of the results are within one standard deviation of the true value, 95·4 per cent within two standard deviations, and 99·7 per cent within three standard deviations. Now the essential point to grasp is that when one makes a single measurement of an activity, it is just like taking a lucky dip into a very large group of such measurements, for the radioactive process goes on all the time, uninfluenced by, and irrespective of, our method of detection, so that in effect the group is there whether or not we take the trouble to record it. Clearly then, if 68 per cent of such a group falls within one standard deviation of the true value and we make a random choice, we have a 68 per cent chance of choosing a value which will be within one standard deviation of the true value. Similarly, we have a 95·4 per cent chance that the true value will be within two standard deviations of our lucky dip and a 99·7 per cent chance that it lies within three standard deviations of our choice.

It is hoped that this illustration will have made it clear that when an activity is written as 15·6 ± 0·1 d.p.m. (i.e. with an ' error ' or standard deviation of ± 0·1 d.p.m.) it means that there is a definite probability of 68 per cent that the true activity lies between 15·5 and 15·7 d.p.m. It does not mean that the true rate necessarily lies between these limits, but only that the odds are roughly two to one that this is so. If one considers limits of two

standard deviations, the chance of including the true value between these larger limits becomes greater; the probability is now 95·4 per cent which means that there is only about one chance in twenty of it being outside the limits of 15·4 to 15·8 d.p.m. For limits of three times the standard deviation, the probability rises to 99·7 per cent, so that in this case there are only three chances in a thousand that the true value is outside the limits 15·3 to 15·9 d.p.m. For the non-mathematical, one could therefore translate an activity of 15·6 ± 0·1 d.p.m. into the following terms: It is *fairly probable* that the true value lies between 15·5 and 15·7 d.p.m. (one standard deviation), *probable* that it lies between 15·4 and 15·8 d.p.m. (two standard deviations) and *highly probable* that it lies between 15·3 and 15·9 d.p.m. (three standard deviations).

The phenomenon of scattering around the true value is of direct concern when it comes to the question of Milojčić's criticism that Libby did not effectively demonstrate that carbon-14 is evenly distributed throughout the carbon exchange reservoir. From what has been written above, it will be appreciated that repeated measurements of the activity of any one sample of carbon from living material would scatter in a particular way about the mean value (which closely approximates to the true value). Libby found that the individual measurements on a series of samples of different materials from widely different geographical locations were scattered about the mean value in the same way as would be repeated measurements on the same sample. He was therefore justified in concluding that, within the limits of experimental error of his measurements, the carbon-14 activity of living material was uniform.[9]

We have seen how the statistical errors inherent in the measurement of radioactivity are expressed. Carbon-14 dating involves measurements of a modern reference sample and the sample being dated, each quantity being subject to statistical fluctuations. The radiocarbon age is therefore subject to the combined effect of these perturbations. Consequently it can never have the same precise, clear cut meaning as a calendar date and, in particular, the published radiocarbon age must *never* be divorced from its error term, which is a standard deviation and must be interpreted as such. For example, a radiocarbon age given as 2710 ± 130 years (standard deviation of ± 130 years) means that the true age lies *fairly probably* (two chances in three) between 2580 and 2840 years or *probably* (nineteen chances in twenty) between 2450 and 2970 years or *highly probably* (997 chances in 1000) between 2320 and 3100.

Clearly there is no high degree of certainty that the true age lies within the limits defined by one standard deviation, and there is still one chance in twenty that it is outside the limits of two standard deviations. However, for ordinary purposes one can say that there is a reasonable certainty that the true result lies within the limits of two standard deviations from the experimental value, and this is a useful point to remember. In the example quoted above, for instance, the age of 2710 ± 130 years referred to a sample of Sequoia wood dated by Libby. Limits of two standard deviations (2450 to 2970 years) would include, practically speaking, the historical age which was given as 2925 ± 51 years. It is important to note that the error term in the historical age is *not* a standard deviation, but due to the finite number of tree rings which were included in the sample in order to provide sufficient sample for the measurement.

In comparing two carbon-14 dates, account must be taken of the standard deviations associated with *each* date. Thus the standard deviation of a sum or a difference of two dates is obtained by taking the square root of the sum of the squares of each standard deviation, and is therefore larger than either of the individual standard deviations. For example, the

[9] Libby, W. F., *Radiocarbon Dating*, p. 17.

difference between 3330 ± 200 years and 3100 ± 200 years is 230 years with a combined standard deviation of $\sqrt{(200)^2+(200)^2} = 280$ years. The arithmetic difference between the two ages in this case is *less* than the combined standard deviation, and one can only conclude that the two ages are not significantly different. If, however, the figures were 3800 ± 200 and 3100 ± 200 years, the arithmetic difference would be 700 years, again with the same combined standard deviation of ± 280 years, i.e. the difference would be written as 700 ± 280 years. In this case the arithmetic difference is more than *twice* the combined standard deviation, and one can say that the two ages are significantly different and that the former is *probably* older than the latter with a certainty of 95·4 per cent. It should be noted particularly that even in this case one cannot be very certain about the actual magnitude of the difference between the two ages, because the standard deviation is quite large compared with the arithmetic difference. In the case where the arithmetic difference lies between one and two standard deviations, e.g. 3500 ± 200 and 3100 ± 200 years with a difference of 400 ± 280 years, one has to bear in mind that in this range, there is still a fair probability that such differences are due to chance alone, so that it is not advisable to draw from them highly significant conclusions as to the relative ages of the two samples.

It is hoped that the reader will now be aware of the inherent limitations of radiocarbon dating in so far as the implications of the statistical error are concerned, and will see that much of the criticism put forward by Milojčić is due to the absence of common ground between the disciplines of archaeology and the physical sciences. With some of his final criticism one cannot greatly disagree, for it is quite obvious that the method is at its best in fixing the broad pattern of a chronological sequence rather than the fine detail. The fixing of individual historical events can never be a matter of absolute precision because of the element of uncertainty introduced into the dates by the very nature of radioactivity itself.

RECENT ADVANCES

The question now arises as to what extent the limitations of the method have been reduced or modified by recent advances in technique and researches into the fundamentals of the method.

Newer techniques have certainly made for much greater accuracy in the measurements of radioactivity, and this in turn results in much smaller standard deviations. However, it is now clear that when the statistical error is greatly reduced, other sources of error, which had previously been obscured by the much larger statistical errors of the solid carbon method, must eventually be taken into account. Thus Craig[10] has shown that the carbon-14 content of wood may vary by ± 1 per cent from its contemporaneous average assay. This is due to the fact that during the complicated biochemical reactions which take place in a plant very slight changes of the ratios of the carbon isotopes may occur. The extent of this co-called ' isotopic fractionation ' effect in the sample is such that it can cause an uncertainty of up to ± 80 years in the radiocarbon age, and if a single modern sample is used as the reference standard a similar uncertainty will also be present from that source. The uncertainty can be eliminated only by the application of a correction factor derived from high precision mass spectrometric measurements, and it is unfortunate that the necessary facilities are not always available (there is no suitable instrument in operation in this country at present). Since many dates had been published before this effect was reported or widely known, there are in existence some dating lists which do not take it into account. It will only be important where the standard deviations are small (say less than ± 100

[10] Craig, H., *Geochim. et Cosmochim. Acta*, **3**, 53 (1953); and *Journ. Geol.*, **62**, 115 (1954).

years), and such values should be regarded with some reserve where there is no concise statement as to how the errors have been computed. Since the Cambridge Radiocarbon Conference in 1956, it has been generally agreed that the manner in which the error term has been derived should be stated explicitly in all publications.[11]

SYSTEMATIC ERRORS

Consequent on the isotopic fractionation effect and other factors, it follows that there are systematic differences in the dates of different laboratories, due largely to the fact that they use different ' modern ' reference samples. The position is complicated by the fact that during the past century a considerable quantity of ' dead ' (i.e. non radioactive) carbon has been added to the atmosphere by the combustion of fossil fuels (coal and oil) which are so old that they no longer contain any radiocarbon. Suess[12] showed that this has caused a slight reduction in the contemporaneous carbon-14 assay compared with what it had been previously (the 'Suess effect'). The result is that if one uses 20th-century wood as a reference standard the calculated dates come out too young. Some laboratories have overcome this trouble by using as their ' modern ' standard wood which is 100 years old or more. A corresponding allowance is then made for the age of this reference sample in calculating carbon-14 dates. In the case of dates published before the effect was discovered, the error it introduces is insignificant where the standard deviations are large as in the case of the early Chicago dates. Where this is not so (notably Groningen), corrections will have to be made. It should be noted that in respect of the Groningen dates, De Vries has held up the publication of corrections and continued to use his original reference sample in order to avoid confusion, since any correction factor published prior to general agreement being reached by the various laboratories as to the best way to eliminate the systematic errors would be liable to revision again at some future date. De Vries's own measurements indicate that Groningen dates are probably about 240 years too young.[13] However, the matter of systematic errors should soon be cleared up satisfactorily, for steps are now being taken to ensure that all laboratories make the necessary high-precision measurements, and it will then be possible to publish correction factors so that all dating results from different laboratories can be compared to the highest possible level of accuracy. It should be emphasized here that this is the only way that accurate corrections can be obtained. Any attempt to derive them from the few published dates on samples which are common to two or more laboratories is bound to produce figures which may be very much in error, because the standard deviations in such measurements are large compared with the quantity which is being sought. Attention is particularly drawn to this point in view of a recent article in which Crowe[14] has fallen into this very trap and compiled a table of corrections to be applied to bring the dates of all laboratories into line with Groningen. His use of this table leads him to some very curious conclusions with which no dating laboratory would agree, and it is emphasized once again that such tables compiled in this way cannot be accurate because of the relatively large statistical fluctuations involved. One should wait patiently until the properly compiled corrections are available.

Perhaps the most important recent addition to our knowledge of radiocarbon dating, is due to De Vries.[15] As a result of high-precision measurements on tree rings he has shown that, when one reaches a certain stage of accuracy, the basic assumption that the activity

[11] Levi, Hilde, Report on the Cambridge Radiocarbon Dating Conference, *Nature*, **176**, 727–8 (1955).
[12] Suess, H. E., *Science*, **122**, 415 (1955).
[13] De Vries, Hl., *Koninkl. Nederl. Akademie Van Wetenscappen, Amsterdam*, Series B, 61, no. 2 (1958).
[14] Crowe, C., *Nature*, **182**, 470 (1958).
[15] De Vries, Hl., *ibid.*

of living material has remained constant throughout the ages is no longer strictly true. De Vries's work indicates that, in fact, the activity has suffered cyclic changes during the past five hundred years, and there is also some evidence of slight geographic fluctuations. This means that there are fundamental limitations on the ultimate accuracy obtainable and it seems that when all other factors are taken into account, errors of 100 years or so may remain from this cause.

OTHER SOURCES OF ERROR

No mention has yet been made of the other factors which are of very definite importance and yet are often overlooked or misunderstood. They relate to the sample itself, its environment, and its chemical pretreatment in the laboratory.

Strictly speaking, the carbon-14 method arrives at the date of death of the organism from which the sample was derived, and it is the task of the archaeologist to relate this event to that in which he is really interested. Considerable error can be introduced by injudicious choice of sample, and it is true to say that a good sampling technique is every bit as important to accurate dating as all the work in the laboratory. For example, a piece of timber might have been dead for several hundred years before it was used for a particular purpose, and this possibility would have to be taken into account in assessing the suitability of a sample for dating purposes. Every effort should be made to select material which allows of this error being reduced to a minimum.

When one considers the environment of a sample, the problem is to decide whether or not this is likely to have affected its carbon-14 content and, if so, whether laboratory treatment can rectify matters. It has already been stated that the process of radioactive decay is not subject to environmental effects, and any doubts about this can therefore be dismissed. The most likely source of environmental error, and one that can assume major proportions, is that due to the addition to the sample from its surroundings of carbon-containing materials of different age, e.g. rootlets and humic constituents of the soil. Whether or not these can be removed in the laboratory depends on the nature of the sample. Libby[16] has pointed out the unique position of charcoal in this respect, for its chemical stability allows one to use drastic reagents, thus facilitating the removal of all but the original carbon which is left behind. Humic acids can thus be removed completely if extraction with caustic soda is sufficiently prolonged, and such meticulous preparation is *de rigeur* when the goal is accuracy of a high order. When there has been insufficient washing with alkali in the first instance, the removal of younger humic material will necessarily be incomplete, and this would seem to be one explanation for Zeuner's findings that more intense washing with alkali of the charcoal from Jericho gave older results.[17]

In considering ways in which the carbon-14 content of a sample may be influenced other than by the direct addition of foreign material, one finds it very difficult to see how any but very minor effects can take place. Thus the selective removal of carbon-14 from the sample, either by the chemical nature of its environment, or by fungal attack, or bacterial decay as postulated by Zeuner[18] must necessarily involve isotopic fractionation processes which, if they occur at all, would almost certainly be too small to be detected directly by by carbon-14 dating methods. There was certainly no justification for invoking such processes when discussing the measurements on the oak pile from the Wallbrook, for the results showed no more than the normal statistical scatter which one obtains on

[16] Libby, W. F., *ibid.*, p. 43.
[17] Zeuner, F. E., ANTIQUITY, XXX, 195 (1956).
[18] Zeuner, F. E., *Eleventh Annual Report of the Institute of Archaeology*, 1955.

repeated measurements of the same sample and provided no evidence whatsoever in support of the processes postulated by Zeuner having occurred.

CONCLUSION

In the present survey, an attempt has been made to restate in simple terms the principles underlying the technique of carbon-14 dating, and to assess the present position of the method in the light of recent criticism. Magnetic measurements cannot yet be said to have afforded sufficient evidence for a convincing demonstration that the radiocarbon method is subject to serious error due to cosmic ray fluctuations in the past. Milojčić's criticism is shown to be based largely on misunderstanding of the basic principles of the method and it is hoped that the present account will help to bridge the gap between the archaeologist and the physicist.

The present state of the method is such that the accuracy of the dates is no longer limited by statistical fluctuations in the measurement of the radioactivity of the sample, but by small perturbations which were previously obscured by the large statistical errors of the original technique. Of these perturbations, isotopic fractionation effects can be eliminated if the necessary high-precision mass spectrometric techniques are available, but De Vries's recent work indicates that when all other errors are taken into account and eliminated, there may remain errors of up to 100 years or more which seemingly cannot be reduced by any foreseeable means.

Because of the fact that the improvements in instrumentation preceded our knowledge of some of these limiting factors, it is inevitable that the accuracy of some published dates is rather too optimistic, and, excepting where it is clear that errors other than the statistical error have been taken into account, one should treat error terms of less than $\pm$ 100 years with some reserve. In such cases, advice should be sought from the laboratory in question.

Systematic errors still exist between the various laboratories due to differences in ' modern ' reference samples, but measurements are in progress which will result in the publication of accurate correction factors. At present, these differences introduce some extra uncertainty into comparisons between the dates from different laboratories over and above that indicated by a consideration of the published errors. However, this effect is probably not very large except in the case of Groningen dates, which De Vries has stated may be over 200 years too young.

Consideration of all these facts leads inevitably to the conclusion that the carbon-14 dating method is at its best in fixing the broad outlines of a chronology rather than the fine detail.[19]

[19] The writer wishes to thank Dr J. H. Plenderleith and Dr A. E. Werner of the British Museum Research Laboratory for their helpful advice and criticism during the preparation of this article.

ANTIQUITY XXXII, 1958

The Minnesota Petroglyph

by GLYN DANIEL

Sixty years ago a farmer called Olof Ohman found, or thought he had found, or caused it to be thought he had found a stone bearing a runic inscription at Kensington in Minnesota. The Kensington Stone, or Minnesota Petroglyph, has been described as the most important artifact from America and as proof that the Vikings got to Minnesota in the 14th century. Others have described it as a modern fake. In The Kensington Stone, a Mystery Solved (*pp. xiv*, 228, 20 *plates, University of Wisconsin Press, Madison*, 1958, 5 *dollars*) *Erik Wahlgren, Professor of Scandinavian Languages at the University of California, Los Angeles, writes what would seem to be the last word in this exciting story of hoax and deception.*

IN 1862 James Russell Lowell wrote a runological essay which was later published in *The Biglow Papers* (*Second Series*); in it, his creation, the Reverend Mr Wilbur, A.M., finds in America a runic inscription which reads the same straight up, diagonally, or upside down, and which he interprets as saying

HERE
BJARNA GRIMOLFSSON
FIRST DRANK CLOUD-BROTHER
THROUGH CHILD-OF-LAND-AND-WATER

and explains as a record of the first smoking of the herb *Nicotiana Tabacum* by a European in the continent of America. Two years later Jules Verne in his *Voyage au centre de la terre* reveals an interest in runes; in a translation published in Boston in 1874 there occurs this passage: ' " It is a Runic manuscript, the language of the original population of Iceland, invented by Odin himself," cried my uncle, angry at my ignorance.'

In 1898 near the town of Kensington, in Minnesota, was found a stone bearing an inscription in some form of the Swedish language carved in letters related to early Scandinavian runic alphabets, together with one word (or set of initials) in Latin lettering, and the date 1362. The finder was Olof Ohman, and he found the stone, he alleged, in the roots of a tree on his farm. Professor Wahlgren gives the following rendering of the runic message into modern English:

> 8 Swedes and 22 Norwegians on an exploration journey from Vinland westward. We had our camp by 2 rocky islets one day's journey north of this stone. We were out fishing one day. When we came home we found 10 men red with blood and dead. AVM save us from evil. We have 10 men by the sea to look after our ships, 14 days' journey from this island. Year 1362.[1]

This inscription, if a genuine historical document, shows that white men not only reached

[1] E. Wahlgren, *The Kensington Stone, a Mystery Solved*, from which all other quotations in this article, unless otherwise specified, are taken, and which contains a complete bibliography and documentation of all statements.

North America, but had penetrated far into the interior of the continent, 130 years before Columbus and 300 years before French explorers, and some three and a half centuries after Leif Erikson and his adventures in Vinland. But is it genuine? In 1899 three Scandinavian runic experts declared that ' the so-called rune stone is a crude fraud, perpetrated by a Swede with the aid of a chisel and a meagre knowledge of runic letters and English ': and for a while the stone was forgotten about. Then, in 1907, a Norwegian-American writer called Hjalmar Holand, while touring Minnesota to collect material for a book on the history of Norse settlements, acquired the Kensington stone, and since then has been campaigning vigorously for its authenticity.

Holand's first work, *De Norske Settlementers Historie* (1908), devotes only eight pages to the Kensington stone. In 1910 a Committee of Seven of the Philological Society of the University of Illinois unanimously pronounced the stone a recent forgery. Holand, nothing daunted, continued his campaign for recognizing the authenticity of the stone; *The Kensington Stone* (1932) is a full treatment of the subject, and in *Westward from Vinland* (1940), *America* 1355–1364; *a new Chapter in Pre-Columbian History* (1946), and *Explorations in America before Columbus* (1956) he develops his thesis that members of an official organization authorized by King Magnus Smek of Sweden and Norway, which ostensibly took place under the direction of Paul Knutson during the years 1355-64, reached Minnesota and that their expedition is recorded on the Kensington Stone. Holand also adduces other archaeological ' evidence ' for his thesis, notably the famous stone tower at Newport, Rhode Island, usually regarded as 17th century in date, but which he claims to be much earlier and Scandinavian. To a certain extent, the story of the Kensington stone is the story of Holand's advocacy of it. G. M. Gathorne-Hardy, in reviewing Holand's *The Kensington Stone* in ANTIQUITY (1932, 420)[2] wrote that ' Mr Holand has made out a case for a complete new hearing, and . . . objection to the stone can no longer be based on the fact that the weight of authoritative opinion has hitherto been adverse '. while the *New York Times Book Review* called *Westward from Vinland* ' probably the most important . . . contribution yet made to the pre-Columbian history of America.'

From February 1948 until February 1949 the Kensington Stone was on exhibition in the Smithsonian Institution, Washington, D.C., and Dr M. W. Stirling, Director of the Bureau of American Ethnology in that Institution, is quoted as declaring that it was ' probably the most important archaeological object yet found in North America '. On 15 March, 1949 the stone was unveiled at St Paul in honour of Minnesota Day on the occasion of the Minnesota centennial, and in the same year no less an authority that Professor Johannes Brøndsted is saying, ' After long and close study, I am personally inclined to believe that this famous runic monument is genuine.' On 12 August, 1951, sponsored by the Alexandria Kiwanis Club, there was unveiled in Runestone Memorial Park at Alexandria, the county town of Douglas County, Minnesota, a gigantic replica of the Kensington Stone, and today in the shops at Alexandria may be bought replicas of the stone and photographs of it as automobile stickers.

Clearly the Minnesota petroglyph has become a cult-symbol of the ancientness of the white man in America, and as Professor Wahlgren observes, the Smithsonian sponsorship has brought about a new era in the history of the stone. But does Smithsonian sponsorship, a gigantic replica and automobile stickers make it authentic? Is there any justification for regarding the stone today as other than the judgments of 1899 and 1910? Is Holand no more than a wayward scholar refusing to believe expert runological opinion, persuading himself of the authenticity of the stone, and also persuading a gullible public ever anxious

[2] In an article entitled ' Alleged Norse Remains in America ' which contains a transcript and a photograph of the inscription.

to find experts discomfited, readily excited by suggestions of Scandinavian antiquity in American, and only too willing to plunge uncritically into the lunatic fringes of archaeology? What is the truth? Here it is set down, soberly, convincingly, succinctly, and with a wealth of scholarship, by Professor Wahlgren. His conclusions are that ' the stone was carved in the 1890s and probably in 1898 by an uneducated, but intelligent Minnesota resident, a ponderer and a seeker, a man of shrewdness and humour '; he leaves us in no doubt that he believes the carver to have been Olof Ohman, but has the same restraint in naming the forger as Dr Weiner had in *The Piltdown Forgery* in naming Dawson as responsible for the whole of that astonishing hoax. ' The planting of the Minnesota stone was a clever and understandable hoax,' writes Wahlgren, ' *not in any sense a dishonourable act*'; and he quotes with approval the dictum of Professor S. B. F. Jansson that ' it did not become a fraud in the true sense until . . . quantities of essays and articles were written in order to prove it genuine '. The Kensington Stone is thus a false antiquity like the Cardiff giant, the Schleswig turkey, the tiara of Saitaphernes, and the Piltdown skull.

But false antiquities are of very great interest to students of genuine antiquities for two reasons; first, their unmasking is an enthralling story of scholarship, archaeology and detection, and secondly, because the circumstances of their creation and acceptance are matters of archaeological history and of the way in which real archaeological knowledge does or does not spread to a wide reading public. Wahlgren's tale of detection must be read by all. It is, in its essentials, the debunking of Holand, and the establishment of the possibility and probability of a hoax by Ohman. He tells us that Ohman studied runes at school and possessed a copy of Montelius's *Sveriges historia* distributed as a supplement to the *Svenska Amerikanska Posten* of Minneapolis between 1897 and 1898, and of Rosander's *Den Kunskapsrike Skolmästaren*, and that pages 61 to 64 of his copy—pages which deal with the history of the Swedish language—are ' much-fingered '. The hoax was a joke in 1898, and not an attempt at making money. Indeed Ohman sold the stone in 1914 to Holand for 25 dollars. But by now this false antiquity has become valuable; in a recent lawsuit at Alexandria, in which three people claimed unsuccessfully that they had bought the Kensington Stone from Holand for 2500 dollars, the court tentatively assessed its value at 25,000 dollars. It reminds one of the American impressario who offered the French Government the equivalent sum of money they had paid for the tiara of Saitaphernes when they believed it genuine, on condition they gave him a certificate that it was the genuine fake!

But even more interesting than Wahlgren's debunking of Holand and unmasking of Ohman, is his study of the circumstances that could have permitted the alleged ' find ' to be described as an authentic discovery. In 1874 Professor Rasmus Anderson, of the University of Wisconsin, published at Chicago his *America Not Discovered by Columbus* which called attention to the prior claims of the early Norsemen. In 1887 Gustav Storm, in his *Studies into the Vinland Voyages*, asserted that Norsemen in the 11th century had never been within the present boundaries of the United States. This assertion created tremendous ill-will in Scandinavian-American circles, and in the decade preceding 1898 there was lively and acrimonious public and private discussion about the Vinland voyages. The quadricentennial Columbian Exposition at Chicago in 1892–3 brought these discussions to a focus. They were further high-lighted by the sailing to America in the spring of 1893 of an exact replica of the Gokstad ship. It left Bergen on 30 April, 1893 and arrived at Newfoundland on 27 May and it is still on show in Chicago's Lincoln Park; 1898 was a moment to establish the antiquity of the ancient Scandinavians in America among the modern Scandinavians of Minnesota. The Kensington hoax is thus, in Wahlgren's words, ' an episode in the history of the development of the American frontier '.

It is even more than that; it is an episode in the development of the lunatic fringe of

archaeology; 1893 not only saw the sailing of the replica Gokstad ship to the Chicago World Fair, but the appearance at the Columbian Exposition of Swami Vivekananda and the establishment of the Vedantic Society the following year. The late 19th century was also the age of Ignatius Donnelly (1831–1901), that eccentric lawyer, ' the sage of Nininger ', Lieutenant-Governor of Minnesota 1860–3, member of Congress, fabulous humorist, whose books *The Great Cryptogram* (1888) and *Atlantis* (1882) have probably done more than any others to attract uncritically minded students of antiquity away from the straight, narrow, and often difficult path of scholarship. In his *Ragnarok* (1883), Donnelly asserts that the Middle West was the ancient centre of a great civilization.

These then are the influences that made the Kensington hoax possible. Its initial purpose seems to have been a joke; ' its author wanted to have some fun with the scholars while mystifying them '. He certainly succeeded in this, as did the painter of the Schleswig turkey, the Piltdown forger, and the man who cut the Mullsjö runes.[3] But jokes get out of hand, and scholars who have committed themselves and their scholarly reputations are sometimes driven, perhaps in spite of their better judgments, to defend positions no longer tenable. This is the great lesson of Wahlgren's book to an age when the enormous popularity of archaeology makes antiquity profitable; any antiquity that has about it the remotest suspicion of being false must be examined as though it were a suspected crime. In discoveries where there is suspicion of fraud (whether or not that fraud is subsequently established), such as Moulin Quignon, Kensington, Glozel and Rouffignac, the police work of an archaeological detective is even more necessary than the subjective judgments of archaeological experts.

Indeed, if the police work of archaeological detection done by Wahlgren in the last few years and recorded in this book, had taken place in the late 1890s, the Kensington stone would long ago have taken its rightful place in the register of false antiquities and fine hoaxes.

[3] Mullsjö is in Sweden and a runic inscription was ' discovered ' there in 1953. This runic hoax provided the only ' inscription ' in runes which was in modern English. The style of the runes is about 1000 A.D. and part of the message reads, ' Joe Doakes went East 1953. He discovered Europe. Holy smoke!'

Book Chronicle—*continued from p. 234*

HISTORY OF THE PRIMATES. *By* Sir Wilfred le Gros Clark. *pp.* 119, 40 *text figures. London: the British Museum* (*Natural History*), 1958. 4*s.* 0*d.* The sixth and fully revised edition of this excellent handbook; invaluable and wonderful value for four shillings.

THE ANCIENT LIBRARY OF QUMRÂN AND MODERN BIBLICAL STUDIES. *By* Frank Moore Cross, Jr. *pp.* 198, 19 *plates. London: Gerald Duckworth*, 1958. 21*s.* The Haskell Lectures 1956–7.

EARLY CHRISTIAN IRELAND. *By* Máire and Liam de Paor. *pp.* 196, 36 *text figures*, 76 *photographs. London: Thames and Hudson*, 1958. 25*s.* Volume Eight in the *Ancient Peoples and Places* series.

THE DISCOVERY OF BRITAIN. *By* Jack Lindsay. *pp.* 281. *London: the Merlin Press*, 1958. 15*s.* A discursive book of essays about local history and field archaeology, mainly in East Anglia. Interesting but hardly justifies the sub-title, ' A Guide to Archaeology '. No index.

[*continued on p.* 292

Notes and News

THE 1957–8 EXCAVATIONS AT WINDMILL HILL

Since it was the late Editor of ANTIQUITY who, with H. G. O. Kendall, first established the Neolithic date of the earthwork on Windmill Hill, and at whose suggestion the major excavations took place, it seems especially fitting that this preliminary account of renewed investigations should appear in this journal. These small-scale excavations were undertaken during the summers of 1957–8 in connection with the work, sponsored by Mrs Alexander Keiller and now in progress, of preparing a full report on the excavations carried out by the late Alexander Keiller, F.S.A., F.G.S., during the years 1925–9. Thanks are expressed to the National Trust and the Ministry of Works for permission to excavate, and to the British Academy for a grant from the Reckitt Archaeological Fund.

Perhaps the most significant result obtained was evidence for Neolithic cultivation and settlement on the hill-top prior to the construction of the camp. This evidence was best preserved under the Outer Ditch bank on the eastern side of the camp, an area not previously investigated. Here the bank still rises to a height of nearly $3\frac{1}{2}$ ft. above the old surface and, wherever its thickness exceeds 15 in., is separated from the undisturbed chalk by a shallow soil on which there remains no trace of a normal stone-free turf and humus zone. After examination of the soil profile, Dr G. W. Dimbleby of the Imperial Forestry Institute reports that it has the appearance of a cultivation soil from which the original turf and humus have been stripped. The suggestion receives support from Dr Dimbleby's success in extracting pollen of a cereal and of weeds of cultivation from samples of the soil. It may be supposed that with primitive equipment (digging sticks or antler hoes) it would be difficult to obtain a tilth in the matted layer of vegetation and roots, and that it would have been more satisfactory to pare this off and till the rubbly soil beneath. This new information about Neolithic agricultural techniques may help to explain other recorded instances of the absence of ' turf lines ' under prehistoric banks and barrows. It is further of interest that at least this part of the Windmill Hill camp had been built over an open clearing, quite recently under cultivation.

A timber structure had also stood on the site of the later bank. It is represented in the small area as yet investigated only by three sockets which would have supported posts about 6 in. in diameter; the sockets are in line and slightly under 4 ft. apart. There is reason to think that a fourth socket may have been totally, and a fifth partially destroyed by a later, but still pre-camp, pit. A circular hearth may belong to the timber structure, which it is tempting to interpret as the end of a rectangular house. The hearth consisted of a flat-bottomed scoop, 1 ft. 9 in. in diameter and 5 in. deep, in which two layers of sarsen fragments had been tightly packed; the upper layer of sarsens was further secured by a layer of rammed chalk surrounding the pit and partially overlying the outer sides of the peripheral stones. The careful construction of the hearth, and the prolonged use, which had reduced the sarsens to a soft, friable condition, imply connection with a permanent dwelling.

A second phase of activity is represented by four clay-lined pits, all within the area which would formerly have been occupied by the ' house '. All the pits are funnel-shaped in section and more or less oval in plan; the largest is approximately 7 ft. long by $4\frac{1}{2}$ ft. wide at the top, and 2 ft. deep. The pits, which may have been water-storage devices, had been hand-filled at the end of their (doubtless brief) period of usefulness. A notable find from one of them is a broken sickle flake—a simple leaf-shaped flake, originally some 5 in. long, with the characteristic diffused lustre along one edge.

Although it is satisfactory to have, for the first time, stratigraphical evidence of a period of Neolithic settlement antedating a causewayed camp, the material found affords no basis for a further subdivision of the Windmill Hill culture. Bones of domesticated animals, pottery and flints (including a leaf-shaped arrowhead) of normal Windmill Hill types lay strewn over the buried surface and in primary association with the hearth and pits. As attempts have previously been made to distinguish an early phase of the culture characterized by the exclusive use of hard, flint-gritted pottery, it is worth mentioning that both flint- and shell-gritted wares have now been found not only on the bottom of a ditch but also beneath the bank.

Unlike the banks composed of puddled chalk reported from other causewayed camps, that on Windmill Hill consists of alternating tips of loose quarried rubble and scrapings of soil and weathered chalk, with merely incidental runs of puddled chalk. It appears to be of simple dump construction, lacking any form of revetment. Nothing was found to indicate the former existence of palisades or other timber-work connected with the bank.

The most important new evidence from the ditch sections is that for the stratigraphical position of Ebbsfleet ware in relation not only to Windmill Hill ware but also to the latest style of Peterborough ware, that which already approaches in form and decoration the overhanging-rim urns of the Bronze Age. In a cutting across the Outer Ditch, here 6 ft. 5 in. deep, sherds of a pot with whipped cord ornament and a characteristic Ebbsfleet rim form were found in a layer of rainwashed silt which immediately overlies the primary silt. A few sherds of Windmill Hill ware and abundant evidence for repeated visits to the camp continued upwards through another 2½ ft. of progressively slower silting to the level, 4 ft. above the bottom of the ditch, where a ' turf line ' had formed. The flints and pottery from the latter deposit were of Late Neolithic types—Beaker, Rinyo-Clacton, and, predominantly, developed Peterborough wares.

But the general character of the finds remained unchanged, and they consisted, as in the layers below, mainly of the broken bones of domesticated animals, so distributed and in such large quantity as to remove any doubt that they were contemporary with the pottery. Thus it appears that, whatever the original purpose of the camp, successive generations of visitors had been engaged in exactly the same kinds of activities within it during the whole of the period represented by the natural accumulation of silt in the ditches, and long after either ditches or banks could have formed effective barriers. Indeed it is beginning to seem doubtful that such was ever their function, at any rate on Windmill Hill.

ISOBEL SMITH

THE WELLCOME TRUST AND ARCHAEOLOGY

Our comments on the Carnegie and Leverhulme Trusts and Archaeology (ANTIQUITY, 1958, 121 and 148) have brought information from Dr Green, Scientific Secretary of the Wellcome Trust, and a copy of the Trust's *First Report* which covers the period 1937–56. During this period the Trust spent £18,148 on completing the work and publishing the findings of the Jebel Moya excavations in the Sudan, of which Sir Henry Wellcome himself had been the enthusiastic leader, and £29,080 on completing the work and publishing the findings of the Wellcome-Marston Archaeological Expedition to the Near East which worked at Tell ed-Duweir, and whose results have just been made available in their completeness by the publication of *Lachish IV: The Bronze Age* edited by Olga Tufnell (Oxford University Press, 1958, £8 8*s*.). The third item of archaeological expenditure listed in this report is £1107 as a grant for the publication of *The Fung Kingdom of Sennar* by the late Editor of ANTIQUITY. These munificent contributions to archaeology cannot unfortunately go on being made by the Wellcome Trustees, although, of course,

they maintain an archaeological section (of which Dr Lacaille is Curator) in the Wellcome Historical Medical Museum. Sir Henry Wellcome made no provision in his Will for the support of further projects in archaeology. ' The only adventure in this field,' writes Dr Green, ' which the Trustees have felt justified in helping in recent years is the fascinating study by Mrs Madeleine Smith, working with Dr Oakley at the British Museum (Natural History), which is aimed at determining the blood groups of the original owners of ancient bones.' Dr Green would like readers of ANTIQUITY to know that the Wellcome Trustees regret that they cannot give further help to archaeologists ' except in such rare cases as that of Mrs Smith, in which there is an overlap between archaeology and biology ', but that they would look ' with sympathy upon any further projects which happen, by however narrow a margin, to extend into their own proper fields of activity as defined in Sir Henry Wellcome's Will '. This is very good news, and a note should be made of the address of the Wellcome Trust: 52 *Queen Anne Street, London W.*1.

LEATHER RESEARCH

Dr Donald Burton, Professor of Leather Industries in the University of Leeds is in charge of an investigation into the age, sources and techniques of manufacture of the Dead Sea Scrolls, and to further this work needs to obtain information of a comparative nature by the examination of similar materials of animal origin, i.e. leathers, parchments and vellums, preferably of known age and provenance, and, more especially, those of post-Roman date. He asks if readers of ANTIQUITY would co-operate in this work by sending him small fragments of such materials; pieces only half an inch square will be adequate for this purpose, but would inevitably be destroyed by the tests carried out on them.

THE ARCHAEOLOGY OF CYPRUS

Dr Paul Åström, Director of the Swedish Institute at Athens, asks for assistance in locating Cypriote antiquities in museums and private collections. In connection with his work for the Swedish Cyprus Expedition he proposes to publish an inventory of Cypriote antiquities. He would be grateful if those who own or have knowledge of Cypriote antiquities (other than the major collections he has already studied) could write to him at the following address: *Svenska Institutet i Athen*, 29 *Jan Smuts* (*Boukourestiou*) *Street, Athens.*

MAGNETIC PROSPECTING

The possibility of detecting buried archaeological features by means of the consequent magnetic disturbance, first investigated[1] by J. C. Belshé at Wattisfield, was tried out during the excavation in March 1958 of a stretch of the proposed route of the new Great North Road near Water Newton (Huntingtonshire) where it borders Durobrivae.

The expectation that the thermo-remanent magnetism of baked clay would produce a strong magnetic disturbance at ground level in the vicinity of a buried kiln was borne out. By means of a magnetic survey the presence of a hitherto unknown kiln was detected beneath the turf of a field devoid of surface indications, and its position was so accurately localized that a test-hole immediately revealed the upper rim of the kiln at a depth of 3 ft. 6 in. below ground level.[2] The disturbance due to other features, such as rubbish pits, though weaker than that due to a kiln, was appreciable and again enabled rapid and precise localization to be made. The disturbance in this case arises from the different magnetic susceptibility of the pit-filling compared to surrounding sub-soil into which the pit was cut.

[1] Belshé, J. C., *Advances in Physics*, 6, pp. 192–3 (1957). The archaeological implications were also discussed by Mr Belshé and one of us (G.W.) at a meeting of the Society of Antiquaries on 13 February, 1958.

[2] Since this kiln lay outside the immediate area of road improvement its excavation has not yet been undertaken.

From the experience gained the method appears to have wide application in archaeology, the main limitation being that the ground surface must be fairly regular and unobstructed, and free from all extraneous iron, however small. Two types of instrument were used, one being a conventional magnetometer of the torsion-fibre type and the other a transistorized proton magnetometer. The latter is the more rapid in operation and with it a 10-ft. mesh of measurements can be made over an area of 1 acre in three hours, though when looking for small features such as pits the closer mesh necessitated trebles this time.

M. J. Aitken
Graham Webster
A. Rees

APENNINE HORNED HANDLES

Among the very varied Apennine horned handles of Bronze Age Italy, one type, and one only, has a secure origin outside the peninsula. It is that in which the short horns are set directly on the apex of a high loop handle of cylindrical section, as in FIG. 1. There are no prototypes for it in Italy, where even this high handle, unhorned, appears late in the sequence, and may well itself be derived from the horned form. With or without horns, it is frequently found fluted, certainly a late trait, and survives well into the Iron Age, at Cumae for example.

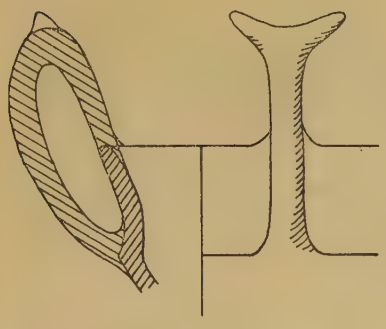

FIG. 1. Aegean type of horned handle (Belverde). × $\frac{1}{6}$.

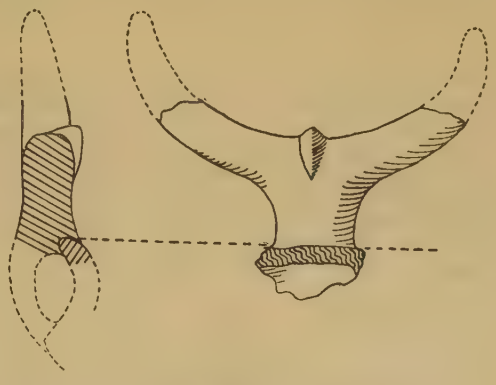

FIG. 2. Italian type (Troy VI–VII). × $\frac{1}{4}$.

The home of this handle is the Aegean, as Säflund pointed out,[1] particularly Troy, where it is found in levels of the later 6th and early 7th Cities. The handles from the two areas are identical in shape: a difference exists in that the eastern ones are usually found opposite a horizontal handle on a goblet or pedestal-cup, whereas in Italy they are invariably single and are on the normal varieties of Apennine carinated bowl. The Apennine folk's taste for elaborate handles, manifested in countless ways through the peninsula, apparently seized upon an alien form which appealed to it and adopted it readily. Where, when and how did this alien form reach them?

The distribution of the earlier unfluted handles points unmistakably to Taranto (see FIG. 3). The Scoglio del Tonno produced twenty-five examples and the neighbouring coastal sites a further six. No other Apennine region has more than eight. Furthermore, all

[1] G. Säflund, 'Punta del Tonno', *Dragma* (*Essays Presented to M. Nilsson*), p. 476 and fig. 27.

FIG. 3. Distribution in Italy of the two types of handle.

except possibly the single examples from Filottrano, Coppa Nevigata and Pertosa can be dated after the collapse of Troy VI, and most after the fall of the Homeric city, when they disappear along with the other forms of the Grey Minyan Ware in the east. Taranto therefore seems the most likely centre for their spread through Italy.

Presumably they reached Taranto during the life of Troy VI or VII A, which closed about 1200; presumably also at a time when imported Mycenean sherds [2] show Taranto to have been in contact with the east, beginning soon after 1400. This gives us a date in the 14th or 13th centuries. It also shows us how they could have travelled, since at this same period Mycenean III A–B wares are also found at Troy. Troy and Taranto were, in fact, outlying ports of a common trading area, an early ' European market '.

A more attractive, though more hypothetical, explanation would be to suppose that

[2] Lord William Taylour, *Mycenean Pottery in Italy*, p. 81.

some pre-Homeric Aeneas had preferred to try his fortunes in the west rather than rebuild his home in the earthquake-shattered ruins of Troy VI. Even more daringly, one could suggest that it was Aeneas himself, or some unsung contemporary of his, who brought this idea to Italy when the Greeks drove him from Troy VII A.

Fortunately for the archaeologist, less so for the romantic, there is more evidence than this, from Troy itself, which supports, confirms even, the more prosaic view. Amongst the material of the Schliemann collection in Berlin is, or perhaps was, a sherd whose unquestionable Italian origin has not to my knowledge previously been noticed. This [3] is a typical Apennine horned handle (FIG. 2). Five more are mentioned in the text, though of the other two illustrated only one seems to belong to the same kind of handle, in a later form. These too come from the VI–VII cities.

In Italy, these handles are primarily northern and eastern in distribution, being commonest in Emilia, though there at a later date, and the Marche. However, they do occur also in smaller numbers at a few southern sites, like Coppa Nevigata and particularly Scoglio del Tonno, witnessing to trade down the east coast of the peninsula. In the 12th century, the quantity of material imported into Taranto from the Po Valley, both terramara and Emilian Apennine, was so great that the Scoglio del Tonno was for long thought to be a true terramara. Yet the origins of this trade clearly go back earlier. It starts indeed as an eddy of the main amber route which connected the head of the Adriatic with Greece, but it later gained importance in its own right, after the collapse of the Mycenean markets, as the means by which the terramara bronze-work was distributed through peninsular Italy.

These handles do not point to Taranto as unequivocally as did the others. On the other hand, they are frequent at that site, whereas positive evidence for foreign trade in the Marche, their main centre at this date, is so far lacking.

Many times during the study of Central and West Mediterranean prehistory, hints, sometimes even more than hints, are found of the import of objects or ideas from the East. The finding of evidence of such an import, with the cross-check of another travelling at the same period and between the same ports, but in the opposite direction, is sufficiently unusual to justify the publication of a note such as this.

D. H. TRUMP

CASTELL ODO

Mr Leslie Alcock, Lecturer in Archaeology at University College, Cardiff, sends the following note about his excavations at the Early Iron Age Fort of Castell Odo in Caernarvonshire:

The ring-fort of Castell Odo, near Aberdaron, at the westerly tip of Caernarvonshire, has recently yielded pottery of great significance for the study of the Early Iron Age colonization of the lands bordering on the Irish Sea. Excavations were carried out by the Department of Archaeology of University College, Cardiff, on behalf of the Caernarvonshire Historical Society, last Easter. The visible defences of the fort comprise two roughly circular banks separated by a wide interspace. A section through the banks revealed that the outer one had been preceded by a timber stockade, set in a stone-lined trench. Associated with this stockade were found over a hundred sherds, including rims from four or five different pots. The fabric of the sherds, a black ware with much grit backing and coated with a brown slip, is comparable to the so-called ' Flat-rimmed Ware ' of the Ultimate Bronze Age in the Highland Zone. One of the pots, however, had a situlate form and finger-dimpled rim which relates it on the one hand to the Iron Age ' A ' pottery of Southern Britain, and on the other to coarse situlate jars from Ireland and the Shetlands. The pottery

[3] H. Schmidt, *Schliemann's Trojanischer Altertümer*, p. 161, no. 3317.

and associated timber defences are clearly the result of an invasion of Caernarvonshire. Nothing is known of the dwellings of this first phase of Castell Odo, except that one at least of them was furnished with an elaborate stone-lined drain.

The palisade was later burned down, perhaps by a second band of invaders who then replaced it with the double ring-work. Eight circular huts with rough stone walls were built inside the inner bank. One of these huts was also excavated last Easter. It produced a sherd of pottery in the same tradition as that of the first phase, together with a very fine saddle quern.

It is hoped to undertake further excavations at Castell Odo at Easter, 1959, when the entrance will be examined and further huts will be cleared. An illustrated account of the work will appear in the next number of the Caernarvonshire Historical Society's *Transactions*.

Anyone interested in taking part in this excavation should write to the Department of Archaeology, University College, Cardiff.

ANGLO-SAXON POTTERY

By holding a week-end conference on Anglo-Saxon Pottery at the Castle Museum, Norwich, from 18 to 20 April, the Council for British Archaeology, stimulated by its Migration and Early Medieval Research Committee, has broken new ground in Dark Age studies in this country. The conference, the first ever devoted to this subject, was attended by nearly fifty archaeologists, most of whom had some special contribution to make to the subjects under discussion. In addition to the English contingent of some forty-four scholars, Prof. F. Tischler, Director of the Niederrheinisches Museum at Duisburg, was an official guest, and he was accompanied by two German colleagues, Dr A. Genrich of Hannover, and Dr W. Winkelmann of Münster, and by one from Holland, Dr J. Ypey of Amersfoort.

Four main subjects were introduced in succession by the principal speakers: English Pagan Pottery by Dr J. N. L. Myres, The Continental Background by Prof. Tischler, Middle Saxon Pottery by Mr J. G. Hurst and Late Saxon Pottery by Mr G. C. Dunning. An hour's discussion followed each of these introductory papers, and in a concluding session the work of the meeting as a whole was summarized by Dr J. N. L. Myres. Welcome entertainment was provided by the Lord Mayor of Norwich who gave a reception in Stranger's Hall, and by the Norwich Museums Committee. Time was also found for excursions to some of the principal sites of the period within easy reach of Norwich, such as the great pagan cemetery at Caistor St Edmund, close by the Roman town of Venta Icenorum, the Roman and Saxon port at Caister-next-Yarmouth, and the late Saxon town of Thetford. The B.B.C. recorded a discussion of the work of the conference which was broadcast in The Archaeologist on 22 May.

In addition a truly remarkable exhibition of some seventy groups of pottery of all periods from the 5th to the 11th centuries A.D. was staged by Mr R. R. Clarke and his staff in the Norwich Castle Museum. They were able to draw not only on their own splendid collections, but on material contributed by many of those taking part from museums and private collections all over eastern and midland England, including exhibits from several important recent excavations by the Ancient Monuments Inspectorate of the Ministry of Works. This exhibition, which was all the more informative for being held in the same room as that used for the meetings, was really the central feature of the whole conference, for the expression of views, formal and informal, on every topic discussed, could be illustrated by the inspection and handling of the relevant objects. It was, incidentally, by far the most comprehensive and significant display of the whole range of Anglo-Saxon pottery

ever assembled in a single room. Photographs of many of the principal exhibits can be obtained on application to the Norwich Castle Museum.

This is not the place to summarize the views expressed or to assess the results achieved. It is hoped to publish a full account of the formal proceedings in *Medieval Archaeology*, and of this reprints will be available as *Council for British Archaeology Research Report No.* 4. As usual on these occasions the informal contacts between those who took part were particularly valuable in suggesting significant comparisons or promising lines for research. A few outstanding points alone can be mentioned here. In the discussion of the earliest material attention was focused particularly on the links between the Roman and Saxon periods provided not only by the hybrid Romano-Saxon wares but by the fact that, at any rate in East Anglia, a small but significant proportion of the purely Germanic cremation pottery appears to antedate the traditional *Adventus Saxonum* in the mid-5th century. The social and economic implications of the development of a specialized pottery industry in the 6th century were also discussed, and the range, distribution, and relationships of the products of some of the principal potters and workshops operating at that time were considered. It also became apparent in discussion that there is at present too little co-operation between the study of pagan and Middle Saxon ceramics so that the links between the pottery of the 6th and 7th century cemeteries and that from the earlier domestic industries of the Christian period, such as Ipswich ware, are imperfectly understood. Prof. Tischler, who, in addition to his own valuable contributions to the discussions acted on occasion as interpreter for his German colleagues, gave an illuminating account of recent advances in Dark Age ceramic studies in North Germany, developing in several ways the argument of his recent important paper on *Der Stand der Sachsenforschung archaeologisch gesehen*.[1] He stressed in particular the difficulties involved in the attribution of special ceramic styles to individual tribal units and emphasized the extent of the tribal intermixture which had already resulted in the development of mixed cultures before the movement to Britain began. Mr Hurst's account of the industries of the Middle Saxon period in East Anglia and the eastern midlands, particularly those based on Ipswich and Stamford, left one eagerly awaiting the application of similar methods to the corresponding material in central and western Mercia, Northumbria, and Wessex, about which so little is at present known. Mr Dunning's magisterial survey of the ceramics of the late Saxon period will not be easily forgotten by those who heard it. He covered both the native industries such as those associated with Thetford, St Neots and Stamford in the east midlands, and the varieties of imported pottery from Frisia, the Rhineland, Belgium and northern France which influenced the later stages of the English development both before and after the Norman Conquest. One of the points which emerged most clearly from this discussion was that in spite of these influences native Saxon traditions in pottery survived the Norman conquest almost unaltered: 1066 is a date of no special significance in English ceramic history.

Among many subjects which received more or less thorough examination in the discussions, or were noted as requiring more intensive study, were the date and significance of Romano-Saxon pottery, the methods of manufacture of Pagan Saxon pottery and the origin and meaning of its stamped decoration, the revival of wheel-made fabrics in Middle Saxon times, the origin of Anglo-Saxon glazes, the purpose and method of manufacture of the sagging base and the extent to which the various local centres of manufacture in Middle and Late Saxon times reflect the changing political divisions of the country. The relation of the English and continental traditions and practices in pot-making throughout the period were also profitably discussed, particularly by the foreign scholars present.

[1] 35 *Bericht der Römisch-Germanischen Kommission* (1956).

The Norwich conference owed something in its inspiration, planning, procedure, and personnel to the highly successful gatherings of the so-called *Sachsensymposion* which, under the enthusiastic leadership of Herr Karl Waller of Cuxhaven and his Arbeitsgemenschaft für Sachsenforschung, have been a notable feature of north German archaeology in the past ten years. Properly followed up it could mark a decisive stage in the development of a backward area of Dark Age archaeological study in Britain. It is indeed startling to recollect how rapidly that development has taken place: twenty years ago the holding of such a conference would have been almost unthinkable for much of the material under discussion was either not known to exist, or lay unrecognized, wrongly dated, or inaccurately described in the storerooms of our museums. Much of it, especially that of the Pagan period, is still quite inadequately published, and the need for a *Corpus Vasorum Anglo-Saxonicorum*, first pointed out in ANTIQUITY in 1942[2] is even more urgent now than it was then. The Council for British Archaeology has performed a most useful service in providing the setting for the assessment of our present knowledge and the stimulus to further advance.

J. N. L. MYRES

[2] *Antiquity* XVI, (1942) 341.

MUSEUMS AND THE BRITISH STATE

At the Twenty-ninth Annual General Meeting of the Museums Association held in London in July of this year, the following motion was passed:

' *That the Museums Association*

(a) *deplores the persistently negative attitude of H.M. Government towards the granting of assistance to the local museums and art galleries in the United Kingdom;*

(b) *urges the Council, the Joint Committee on Government Assistance and individual Members of the Association to do everything in their power to bring the seriousness of the situation to the notice of Members of Parliament and to the general public; and*

(c) *believing that H.M. Government will be the more readily persuaded to assist local museums if they are banded together in strong Regional Groups with a united purpose, gives its encouragement to Federations throughout the United Kingdom to form, as a matter of urgency, Regional Museum Services.*'

The proceedings of this Annual General Meeting are printed in the August 1958 issue of the *Museums Journal*, which also contains the speech of Sir Mortimer Wheeler on ' Government Assistance and Regionalization ', in which he proposed the motion we have quoted and described the Standing Commission on Museums and Galleries as ' an archaic ornament rather than an active instrument of reform '.

We draw the attention of all readers of ANTIQUITY to this important resolution of the Museums Association and urge them to do everything in their power to see that Museums and Art Galleries in Great Britain should become, in the near future, recognized for what they are, one of the most valuable agencies of education and pleasure in the state. Two recent short publications may refresh the minds of those interested in this extremely important problem. The first is *The State and the Arts*, by Lord Bridges, being the Romanes Lecture given before the University of Oxford in June (Oxford University Press, 2*s.* 6*d.*), in which he discusses, among other things, the case for a Minister of the Fine Arts, and for a quinquennial grant to Museums, with a Museums Grants Committee on the same lines as the University Grants Committee. The second is the Bow Group *Memorandum on the Museum and Art Gallery Service*, with its neatly tabulated facts and its

recommendations of (i) a Royal Commission; (ii) a Central Advisory Body; (iii) a block grant, and (iv) a Regional Scheme for Museums. This interesting pamphlet can be obtained from *The Bow Group, 22, St Giles High Street, London W.C.2.*

INTERNATIONAL CONGRESS OF CELTIC STUDIES

An International Congress of Celtic Studies sponsored jointly by Trinity College, Dublin, The Royal Irish Academy, University College, Dublin, and The Dublin Institute for Advanced Studies, will be held in Dublin from 6 July to 10 July, 1959. The work of the Congress will be divided into two sections; the first will discuss the impact of the Scandinavian Invasions on the Celtic-speaking Peoples between A.D. 800 and A.D. 1100 from the points of view of languages, literature and folklore, history and institutions, onomastics and archaeology; in the second, communications directly relating to Celtic languages, literatures or civilizations outside the special topic of the first section will be received. Those wishing to attend this Congress should write to *The Secretary, International Congress of Celtic Studies, Royal Irish Academy, Dawson Street, Dublin.*

ARCHAEOMETRY

Attention is drawn to *Archaeometry*, the Bulletin of the Research Laboratory for Archaeology and the History of Art in the University of Oxford, of which an account was published in an earlier issue of ANTIQUITY (June 1958, 123). It is a duplicated journal edited by Dr E. T. Hall, and its purpose is not to by-pass the normal channels of publication but to provide a rapid means of circulating the results of completed research, to record partially successful projects which are not worthy of normal publication, and to give interim reports on some of the work in progress in the laboratory. The first number, published in the spring of 1958, has, among other articles, an account of Magnetic Dating which will be read with interest by readers of the article on this subject by R. M. Cook and J. C. Belshé in the last number of ANTIQUITY (September 1958, 167) and an account of the Water Newton Survey, referred to briefly in these pages in the note by Messrs Aitken, Webster and Rees on Magnetic Prospecting (*supra*, p. 270). In his foreword to the first number of *Archaeometry*, which we welcome (although we find it hard reading for one untrained in statistics and the natural sciences) Dr Hall writes ' Anybody, whether scientist or archaeologist, who finds the contents of interest, will be willingly added to the circulation list if he applies to the laboratory '. The address of the laboratory: 6 *Keble Road, Oxford.*

TRAVAUX DU LABORATOIRE D'ANTHROPOLOGIE . . de RENNES

Attention is drawn to the *Travaux du Laboratoire d'Anthropologie Générale et des Musées Préhistoriques de la Faculté des Sciences de Rennes*, a duplicated Crown quarto publication edited by Dr P–R. Giot and, at the date of writing, only available as an exchange publication. The first volume published in 1956, contains short articles by J. L'Helgouach on the Gallery Grave of Essé, and ' Les dolmens du type " Loire " en Bretagne ', and a publication of the Treboul Hoard by J. Briard. The 1957 volume contains an important memoir by J. Briard and J. L'Helgouach entitled ' Chalcolithique, Néolithique Secondaire, Survivances Néolithiques a L'Age du Bronze Ancien en Armorique ' which won the first prize offered by the Groupe Finistèrien d'Études Préhistoriques. Those interested in exchanging publications should write to *Dr P–R. Giot, Laboratoire d'Anthropologie, 2 Rue du Thabor, Rennes, Ille et Vilaine.*

Reviews

THE DECIPHERMENT OF LINEAR B. *By* JOHN CHADWICK. *Cambridge University Press*, 1958. *pp.* xi + 147. 18*s.* 6*d.*

Michael Ventris' achievement has excited so much interest that it is excellent to have a popular account of it written by John Chadwick, who joined him just after he had broken the code in 1952 and worked with him until his tragically early death in 1956. All will welcome the chance to learn a little more about *ce jeune homme, grand, beau, habitué des sports*,[1] who captured affections by his gaiety and modesty as universally as he won admiration for his combination of linguistic flare, visual memory, sympathetic appreciation, administrative ability, and sheer genius.

Dr Chadwick sketches in the historical, linguistic, and archaeological background and then discusses the discovery of tablets at Knossos and describes the various other scripts which are related to Linear B, such as Linear A, Cypro-Minoan, and the classical Cypriote syllabary. He then notes early attempts at decipherment ending with the three great American contributions: Alice Kober's establishment of gender and inflexions (some of which in fact turned out to be towns and their adjectives), Blegen's discovery of tablets at Pylos, and Bennett's establishment of the signary and elucidation of the weights and measures.

Ventris' essential contribution (after the early article in 1940 and the mid-century report) was the series of twenty work-notes in which he continued the analysis of the tablets. Frequencies helped to distinguish signs representing vowels from signs representing consonants *plus* vowels. Alternative spellings showed what signs must have close or identical phonetic values. The Pylos tablets in particular made it possible to establish various types of inflexion. Using all these clues Ventris was able to construct a grid in September 1951 (Worknote 15). This plotted 51 signs under 5 vowels and 15 consonants. No values were allotted as yet. A glance at the ranks and columns shows how far he had got: one of the vowel columns and seven of the consonant ranks were completely right; the rest had from one to three signs wrong except for vowel column 4, which was a shambles. The grid of February 1952 (which is much easier to understand in *Documents*, p. 20, fig. 3, than in Mr Chadwick's tabulation, where the signs are converted into unintelligible numbers) plotted 58 signs and suggested values for four of the vowels; much was right, but, curiously, although both the *-o* and the *-i* column were largely right, Ventris identified the pure vowel *o* as *i*. All this time he believed that the language would prove to be Etruscan, but the preceding worknote (16) had pointed out the likeness of one declension to the Greek nouns in *-eus* (which it later turned out to be).

The leap to Greek was taken reluctantly and deprecatingly with Worknote 20 on 1 June, 1952. He made two essential moves here; having already established *a* and *ja*, he now established *o* and *jo*. Secondly, he saw that a number of entries on certain tablets were towns and adjectives from towns, as on contemporary tablets from Ugarit. This enabled him to identify Kober's ' declensions ' as Cretan towns with their masculine and feminine adjectives. At this stage the tablets began to talk Greek, and Dr Chadwick was called in to help. In November 1952 the manuscript of 'Evidence for Greek Dialect in the Mycenaean Archives' was sent to the *Journal of Hellenic Studies*. This established the values of 65

[1] P. Chantraine, *Bulletin de L'Association Guilleaume Budé*, 1957, 115.

signs; Dr Chadwick's latest table in this book adds *za*, *mu*, pu_2, ra_3, pa_3, *ju*, and substitutes *su* for qo_2, *du* for da_2, *zu* for zo_2, and *nwa* for nu_2; all of these except *ju*, *du*, and *zu* were already in Ventris' revised table published in the *Bulletin of the Institute of Classical Studies* in July 1954. There also Sign 34 was identified as ai^2 and this has been confirmed by the 1957 Pylos tablets, which also make *dwe* likely for Sign 71. Old suggestions which should have been included are ja^2 for sign 82 and si^2 for sign 85; a likely new suggestion by O. Landau (*Mykenisch-griechische Personennamen*) is mi_2 for sign 22.

Dr Chadwick gives a full account of ' Evidence ' and of its reception and of the remarkable early confirmation provided by the Tripod tablet. This was discovered ' with 400 other tablets ' soon after Worknote 20 was written, but was not cleaned until the winter and was not read until after Blegen had returned to Athens in March 1953, bearing with him the proofsheets of ' Evidence '.[2] In August 1954, as Dr Chadwick records, Ventris' lecture to the International Classical Congress at Copenhagen was a triumph, and when he showed the tripod tablet the whole audience burst into applause. Ventris' paper has now been published and with it the tributes then paid to him by Gelb, Georgiev, and Chantraine.[3] The chair at this session was taken by Björck, who, with Furumark, had been among the first to welcome the decipherment and had proposed Ventris for an honorary degree at Uppsala.

The chapter goes on to describe briefly *Documents in Mycenaean Greek*,[4] and deals firmly and convincingly with the few criticisms of the decipherment which have since been published. Palmer in a recent article [5] has defined the problem very neatly by saying that Ventris committed himself to a precise set of predictions of great complexity. The predictions were made essentially on the Pylos declensions and the Knossos place names; they held true for the considerable body of Knossos and Pylos tablets already published in 1952. Since then they have been tested on the very varied subjects of the tablets found in Pylos in 1952, 1953, 1954, 1955, and 1957, the tablets rediscovered at Knossos in 1955, and the tablets discovered at Mycenae in 1952, 1953, and 1954. They have stood the test magnificently as the facts quoted above show: only one sign of the list in ' Evidence ' was wrong (qo_2); *du*, *zu*, *nwa* are better as standard transliterations than the earlier values but in certain contexts the earlier values are right; eleven comparatively rare signs (or twelve if we add the newly recognized sign for *dwo*) have now been given probable values. Alternative spellings have of course been interesting from the first and contributed to the decipherment, e.g. *du* was first labelled da_2 because it appeared as an alternative spelling for *da*. The new material has shown a greatly increased range of alternatives and it should gradually be possible to discover what limits these impose on interpretation; for instance chariot-fitters appear as *e-qe-a-o*, *a-to-mo* at Knossos, *e-qe-o*, *a-to-mo*, *i-za-a-to-mo*, and (I believe) *i-qi-ja-a-to-mo* at Pylos. This is a daunting range of spellings for what must essentially be the same word, and we are beginning to see what variations are possible and where we must reject an interpretation based on too violent an infringement of the spelling rules. Criticism of the decipherment will continue, as criticism of the decipherment of Akkadian, Hittite, and Ugaritic continues; it will serve the useful purpose of warning off from these very difficult texts those who cannot detect the various kinds of prejudice on which such criticism is based.

Criticism of interpretations is an entirely different matter and is the only method of progress. Dr Chadwick proceeds with a chapter on Life in Mycenaean Greece, which is

[2] C. W. Blegen, *E.A.*, 1952–4, 60.
[3] *Acta Congressus Madvigiani*, I, 69.
[4] Reviewed ANTIQUITY, XXXI (1957), 4f.
[5] *O.L.Z.*, 53 (1958), 101.

eminently sensible and readable. The main lines could be drawn with the first big instalments of Knossos and Pylos tablets,[6] but a lot of fascinating detail on armaments, craftsmen furniture, spices, sheep, and tribute are now added. Palmer's Indo-European origin for the structure of Mycenaean society is rejected, but the structure itself, as drawn by Palmer, remains; his barons have become priests, but Dr Chadwick is wise enough to note that secular and religious titles may coincide. I do not find him entirely satisfactory on religion, but our whole view of early Greek religion needs reassessment with the help of Mycenaean art, the tablets, and later survivals (religion is extremely conservative).[7] Here I would only note the following points. Ares is unjustly suspected (the Lesbian form is late like the Corinthian Poteidawon). ' All the gods ' probably means all the minor gods, as Stella has seen. To say that ' we can only understand Demeter's name as used by a figure of speech for corn land ' is at least clumsy; the element and the deity were not distinguished here; more than six hundred years later Solon could still write (24D): ' greatest mother of the Olympian gods, black earth, from whom I removed the boundary stones '. Earth may perhaps be the ' mother of the gods ' on another Pylos tablet, but other solutions are possible. Dr Chadwick suggests identifying Potnia with the female deity known from Minoan and Mycenaean monuments and warns us against identifying the classical Demeter with this figure; but neither identification nor warning seem to take account of the following facts: (1) Minoan and Mycenaean monuments show not one but several female deities with different attributes; (2) the tablets give (*a*) at least five different Potnias and (*b*) Athena, Artemis, Demeter, Hera, Eileithyia, as well as other less known goddesses; (3) in classical times all these and others could have been called Potnia. There were, therefore, always many Potnias, some of them connected with particular places, and many female deities, who were called Potnia for particular purposes; and if the monuments show us a Mycenaean Athena, Artemis, Demeter, and Hera, as they do, we can suppose that the Mycenaeans gave them these names and sometimes called them Potnia too or instead.

The chapter ends with an excellent assessment of Mycenaean literacy and a cautious account of the light cast by the tablets on Homer. It is perhaps unfortunate to suggest that ' there are two schools of thought: those who believe that the Mycenaean element in Homer is great, and those who think it is small '. Homeric scholarship has long been bedevilled by similar unreal dichotomies, e.g. into Unitarians and Separatists. What the tablets have done for Homeric scholarship is of immense value; they have given us the Mycenaean names for people, animals, things, and institutions, and they have told us the state of the language in the Mycenaean age; thus many elements of subject matter and language in Homer can now be labelled Mycenaean and related to a civilization instead of merely being called ' very old ' or ' early ' as distinct from ' recent ' or ' late '.

The last chapter reviews the prospects. There is always a hope of more tablets; more were found in Mycenae last spring, and Iolkos, where the excavators believe that they have identified the separate palaces attributed by legend to Pelias and Neleus, is said to have produced a vase inscribed with numbers in Linear B. Whether more tablets are found or not, with the present material and the continuing yield of archaeology, it should be possible for historians, linguists, and archaeologists, if they unite their different skills generously, to draw a clearer picture of that intercommunicating civilization of the Eastern Aegean which lies at the base of so much both in the Graeco-Roman and in the Hebrew tradition. This book is the kind of introduction which will not only create wide-spread goodwill for Mycenaean studies, but may inspire the young specialist to tackle unsolved problems.

T. B. L. WEBSTER

[6] Cf. ANTIQUITY, 1953, 196 (Chadwick); 1955, 10 (Webster).
[7] Cf. now K. Kerenyi, *Diogenes*, **20** (1957), 1, etc.; L. A. Stella, *Numen*, **5** (1958), 18.

MEDIEVAL ENGLAND: AN AERIAL SURVEY. *By* M. W. Beresford and J. K. S. St. Joseph. *pp.* xiv and 274. *Cambridge University Press*, 1958. 45*s*.

This is a volume which it is particularly appropriate to notice in this journal. The late O. G. S. Crawford was pioneering when in 1924 he published *Air Survey and Archaeology* and in 1928 (with A. Keiller) *Wessex from the Air*. From the beginning, too, he found a place in Antiquity for articles (like those of E. C. Curwen) which used air photography to bring into relief the indelible traces left by past generations upon the face of the countryside. Developed in one war and further refined in a second, air photography has become, largely through Crawford's inspiration, an indispensable weapon in the armoury of archaeologists, historians and historical geographers.

This second of the Cambridge Air Surveys is an example of its sharpness. The authors themselves define very clearly its utility. In particular it is an aid in the understanding of past economic activity, of men's use of their physical environment. Air photographs give to this study a ' clarity and comprehensiveness ' which documents seldom give; and they make ' explicit and actual ' such abstractions as ' open fields ' or ' road systems ' in the pre-cartographic age. Not only that, however, for a procedure for interpreting air photographs can be worked out by comparing them with old maps, and then this procedure can be applied to the interpretation of photographs where no such maps have survived. In that respect air photographs are a positive addition to the evidence for the English economic landscape between the Anglo-Saxon settlements and the time of the Elizabethan map-makers. It is with the villages, fields, towns, industrial remains, etc. of this period that this volume is concerned.

The method is to discuss generally a specific problem (field lay-out, village forms, town plans and so forth), to illustrate each problem with air photographs of particular examples, and to discuss each photograph in connection with such documentary evidence (including early maps) as may happen to have survived. How much air photography can add to the depth and breadth of our understanding of the medieval landscape is graphically exemplified. It may be added that the format of the volume enhances the profit of using it. Printing and illustrations conform to the usual high standards of the Cambridge University Press. At the same time a two column lay-out enables 111 photographs and 28 maps and plans of varying sizes to be accommodated economically in close juxtaposition with the discussion they provoke. There has to be little turning of pages: text, photograph and diagram can be more or less simultaneously before one's eye.

If this is an excellent illustration of how much air photography has to contribute to the understanding of medieval England, its authors are no less clear about some of the limitations of their technique. By itself, for example, as they point out, it can record only ' the *faits accomplis* rather than the steps in the progress of early settlement '. They show, of course, with great skill how documentary evidence can be utilized to redress this unbalance; but such evidence has not always survived and is almost absent for the earlier phases of settlement. In connection with two ' problem pictures' at the end of their book they recall Sir Thomas Browne's injunction that, about some matters, ' there can be no clear and assured decision without . . . subterraneous enquiry '. Archaeological exploration of deserted medieval village sites is only beginning. Similar exploration at Old Sarum might teach us something more of early medieval towns; and at Stonar, Hedon or Torksey of medieval maritime or inland ports. It may well be that not the least important result of this book will be the encouragement it will give to such bodies as the Society for Medieval Archaeology and local antiquarian societies to initiate a study of the physical remains of medieval England as thorough as that which has been devoted to the remains of Roman and pre-Roman times.

Edward Miller

LES AMPOULES DE TERRE SAINTE. *By* André Grabar. *Paris. C. Klincksieck. pp.* 70, 56 *pls. Price* 3,120 *francs.*

These small silver vessels, made in the 6th century to contain holy oil, or even perhaps the waters of Jordan, have long been known; the majority are preserved in the Cathedral treasuries at Bobbio and Monza. But this is the first time that they have been published fully, as a whole. The very excellent plates in this small volume serve to throw rather a new light on them, for they show that the relief decorations they bear are not merely of iconographical interest, but also, at times, of real artistic quality. They are to be assigned without doubt to Palestine, and they thus constitute perhaps the most important corpus on which to formulate ideas as to the character of Palestinian art at the time. The designs were, in Grabar's opinion, ultimately based on large-scale wall paintings, the prototypes for the various scenes probably being associated with the places and buildings to which they most closely relate. The Ascension is the most usual among them, but the Annunciation, the Visitation, the Nativity, the Adoration, the Baptism and the Marys at the tomb also appear, and on a few examples the decorations are of a formal character, consisting of crosses or figures in the Orans position.

The text begins with a brief survey of the material. The examples that are illustrated are then described in detail, sixteen of them being at Monza and twenty at Bobbio (pp. 15–44). There then follow sections on the art, the iconography, the use and, finally, the importance of these flasks to the archaeologist. All sections are authoritative, and the booklet is to be welcomed equally for its text and the excellent illustrations.

D. Talbot Rice

AKRAI. *By* Luigi Bernabò Brea, with the collaboration of Giovanni Pugliese Carratelli and Clelia Laviosa. *Catania: Societa di Storia Patria per la Sicilia Orientale,* 1956. *pp.* 187, 40 *plates,* 64 *figures.*

Professor Bernabò Brea has given devoted service to the archaeology of Sicily and students of the antiquities of that island should be grateful for this compendious book. Akrai was a small Greek city in the interior of south-east Sicily, founded about 663 B.C. as an outpost of Syracuse. It survived the Roman conquest of the later 3rd century B.C., had a Christian period, and was presumably destroyed by the Saracens in the 9th century A.D. Its successor, Palazzolo, was built a little way to the east and robbed the ancient site of most of its building stone. In the early 19th century Judica uncovered some of the monuments and much of the Greek cemeteries; later Orsi made unpublished trials, and in 1953 Brea began more deliberate excavation.

The ruins themselves, though not spectacular, are interesting. There are substantial remains of a small theatre and a council house, traces of temples and the town wall, and a curious set of rock-cut reliefs representing some goddess and known as 'i Santoni '. For future work the most useful objective seems to be the Greek houses.

The book is straightforward and leisurely. It begins with an account of studies of Akrai, its location, the prehistoric remains, and historical sources. Next come descriptions of the town wall, the town plan, the theatre, the council house and neighbouring structures, water supply, quarries, ' i Santoni ', the cemeteries, and notes on the territory of Akrai. Finally, there are catalogues of architectural and sculptural finds and of inscriptions. The painted Greek pottery is not listed, but then the records are largely defective. Much has been published before in one work or another, but this new assemblage is very convenient. Brea gives the evidence, but is too honest to force conclusions. The illustrations are numerous, though the photographs are unworthily printed and some of the references are wrong. But this is an auspicious beginning to the third series of monographs on Sicilian archaeology.

R. M. Cook

A HISTORY OF THE ANCIENT SOUTHWEST. *By* HAROLD STIRLING GLADWIN. *Bond Wheelwright Company, Portland, Maine,* 1957. *pp.* 383, 175 *illustrations*, 21 *maps*. $8.50.

The author of this book has long been an important figure in archaeological research in the south-western United States. As founder and director of the Gila Pueblo Archaeological Foundation in Globe, Arizona, Mr Gladwin sponsored significant field investigations throughout the South-West. Much of our present knowledge of the Hohokam and Mogollon cultures, in particular, is due to the activities of Gila Pueblo, now no longer operated as a research institution.

In his own interpretive writings, Gladwin has never been one to swim with the current. He is noted for his highly original and controversial views, asserted with boundless enthusiasm, ingenuity, and wit. Many of his ideas have both stimulated and exasperated his more sober-minded colleagues. This book is true to form. It is by no means an orthodox summary of South-West prehistory, but a novel and imaginative interpretation of the archaeological data. The book opens with a description of Gila Pueblo and the work carried on there. Then Gladwin traces century by century the events of south-western prehistory as he sees them. Maps and short summaries accompany each chapter. A wealth of well-chosen drawings and photographs supplements the text.

As Gladwin sees it, the South-West (comprising the states of Arizona, New Mexico, southern Utah and southwestern Colorado) was populated by the time of Christ by tribes of hunters and gatherers. Agriculture, pottery, pithouses and other advanced traits were introduced by two distinct groups of migrants, ' Farmers ' and ' Reds ' (the latter so named from the colour of their pottery). The ' Farmers ' came from Asia via Bering Straits and the Great Plains and settled in the northern part of the South-West about A.D. 200. The ' Reds ' came up from Mexico and moved through the southern section starting about A.D. 500, mixing with the older hunting and gathering groups. Some ' Reds ' got as far as the ' Farmer ' villages. The resulting blends of all these groups in various proportions gave rise by A.D. 800 to the regional variants of what are better known as the Anasazi and Mogollon, or Puebloan, cultures. A third migration in the 8th century brought the Hohokam to southern Arizona from west Mexico. These various groups prospered and developed in peace until about A.D. 1000. Then bands of Athabaskan-speaking nomads, ancestors of the Navaho and Apache, swept in from the east, raiding and plundering the defenceless farming settlements. At first the harassed agriculturists gathered for protection into large villages, often in defensible locations. Spectacular ruins such as Pueblo Bonito and the cliff-dwellings of Mesa Verde date from this period. But resistance was usually hopeless and entire regions were abandoned one by one to the Athabaskan enemy. By 1450, the survivors were concentrated in the few locations where they are found today, the Rio Grande Pueblos and the Zuni in New Mexico, the Hopi in northern Arizona, and the Piman-speaking descendants of the Salado in southern Arizona. The Hohokam had fled to Sonora in Mexico.

The story is delightfully written, clever, and set forth in great detail. Yet other South-West specialists will find few points to which they can give their unqualified agreement. Some data are distorted to fit the scheme; data which do not fit are ignored; much is over-simplified. Gladwin stoutly maintains that culture traits are transmitted almost exclusively by migration. Trade and the processes of diffusion play little part in the scheme and Gladwin simply does not believe in independent invention. The ' Reds ' and ' Farmers ' are devised to explain the distribution of traits which others explain by diffusion and local development. The ' Reds ', in fact, are little more than a pottery type. For his chronology, Gladwin places great reliance on the precision of tree-ring dates, many of which were

determined by himself and in some instances are centuries away from the dates believed correct by other authorities. Tree-ring dates give the appearance of precision, but unless used judiciously and in company with other evidence can be very misleading. Beams were often used and reused for centuries after they were cut. Moreover, not all wood specimens give dates of equal reliability.

But, while many details of the plot must be dismissed as a ' Just-So ' story, this book, like all of Gladwin's writings, contains ideas to be grappled with. In a day when many South-West archaeologists are engrossed in analyses of minor details, Gladwin treats the prehistory of the entire South-West as an inter-connected whole. The cultural continuities and discontinuities and the regional inter-relationships are pointed up, often brilliantly. The insights which lie behind the many dubious details deserve the most careful and serious consideration of every open-minded student of the area.

This book should not be used as a reference for factual information. It will be best appreciated by those with some background in South-West archaeology. The format is unusually good and the illustrations alone are worth the purchase price.

WILLIAM R. BULLARD, JR.

ON SOME BUCKETS AND CAULDRONS OF THE BRONZE AND EARLY IRON AGES. *By* PROFESSOR C. F. C. HAWKES and MISS M. A. SMITH. *The Antiquaries Journal (London)*, *xxxvii* (1957), 131–198.

Bronze sheet metal vessels are amongst the distinguishing characteristics of the European Late Bronze Age whether viewed from a technological, or a more general, cultural and chronological, standpoint. The authors of the substantial paper under review have used an unpretentious title to cloak a study of the first importance not only on the vessels selected for detailed description, but on the wide implications that these bear for connections between the Mediterranean world and barbarian Europe in, and after, the 8th century B.C.

The basic study on sheet bronze vessels in the British Isles was that by E. T. Leeds in *Archaeologia* for 1930, and the lapse of quarter of a century has naturally brought to light further examples, and given a fuller knowledge of the comparative continental material. Professor Hawkes and Miss Smith are now able to present a hitherto unknown bucket from Nannau, Merioneth, which they show to stand at the head of the bucket series in these islands being an import from the west-central European Urnfield Culture during its last phase. They are able to point to a very similar vessel from Ireland, and to demonstrate the adaptations, especially in handle forms, and developments, especially in foot construction, that the insular bronze smiths then effected, as in, for example, the well known Heathery Burn, and Cardross, buckets. With regard to cauldrons, the most important new example, since Leeds' paper, is that from Colchester, a vessel which belongs to neither of Leeds' two classes, standing closer to Greek prototypes, and being possibly parental to Leeds' Class B.

Of later cauldron types, a vessel found in the Thames at London is now recognized to have been an import, probably from southern Germany, of the Late Hallstatt Culture (Ha D), and this may have been the normal type in use in these islands between the manufacture of the Late Bronze Age forms, and the Late La Tène types introduced in immediate pre-Roman times. Such are the bare essentials about the principal vessels discussed, and the authors' descriptions, and commentary on cross-borrowing of techniques and devices, as between the mid-continental bucket tradition and the Atlantic cauldron tradition, demand the closest attention.

It may be more useful here to devote space to a brief appreciation of the chronological, and even historical, implications that are now seen to emerge. The present study makes it

clear that the bronze bucket industry in these islands, most probably centred in Ireland, arose from the direct importation of continental vessels of that type of *Kurd Eimer* which von Merhart has shown to belong to the last phase of the Urnfield Culture (*jüngere Urnenfelderzeit*, otherwise Hallstatt B). The dating of the Nannau bucket, and one of those from the Dowris hoard, can therefore be assigned to the second half of the 8th century B.C. (We prefer the well-known, if corrupt, place-name ' Dowris ' rather than the obscure and alien ' Whigsborough ' to which the authors have given first choice in their designation of this important Irish find-complex.) The starting point of the imported buckets from within the Urnfield Culture province was, with little doubt, the West Alpine area whose connections with the British Isles are well recognized and become ever more substantial. At this juncture the full significance of the Heathery Burn hoard comes into view, for in this, as the authors show, a bucket, early in the series of insular manufacture, was in association with waggon and harness fittings that are of Late Urnfield types—certainly uninfluenced by the true Hallstatt (Ha C, or Ha I) types that came to be so rapidly spread by the wielders of long iron swords.

Although the authors are not primarily concerned with the question of how the Urnfield technology was carried into Ireland and Britain, they do comment on the ceramic associations of the Heathery Burn bronzes, and related finds. Particularly welcome is the illustration of the shouldered potsherds from Heathery Burn, and it is clear that the unfortunate term ' Flat Rimmed Ware ' has not now long to live. The Heathery Burn bucket possesses, of course, ring handles and attachments of a type not known in the continental *Kurd Eimer* series, but which belong properly to the western cauldron tradition, and it is on the history of these latter utensils that the authors have been able to provide even greater enlightenment. Whereas Leeds, on the information available to him, had been misled in deriving the insular buckets from Etruscan forms of the 7th century B.C., his appreciation of the ultimate Greek origin of the great globular cauldrons was correct, and is now more fully substantiated. A distinction must be made between the Greek *lebes*, a relatively shallow vessel going back to Minoan-Mycenaean times, and the *dinos*, a newcomer of the 8th century B.C., being a globular vessel composed of riveted sheets of hammered bronze, and with ring handles. The *dinos* is the prototype of the Atlantic cauldron series, but, itself, is derived from Oriental forms originating in Urartu before that kingdom's downfall at the hands of Tiglath Pileser III of Assyria. The principal Urartian trade outlet to the Mediterranean was at the mouth of the Orontes in northern Syria, and by this means were conveyed ring-handle and sheet-riveted cauldrons to Greece, but only before the Assyrian closure early in the 6th decade of the 8th century B.C. The Greek trade-drive to the western Mediterranean, and especially to the Gulf of Lions, which had become economically necessary at this time brought with it this first flood of Orientalizing styles. No great time-lag was involved, for the Atlantic cauldrons, both of A and B classes, show features that belong to the early phase in the propagation of the *dinos*, features which do not occur in the more developed Greek forms such as were made from the mid-7th century.

These considerations are of the greatest significance for the establishment of insular chronology, and the authors propose a date at the turn of the 8th and 7th centuries for the making in Ireland of cauldrons of Leeds Class A; the Class B cauldrons being developed in the first half of the 7th century. This bracket agrees admirably with the bucket chronology achieved on independent grounds, and together they establish the beginnings of the insular sheet metal industry by the end of the 8th century B.C.

Attention must also be drawn to what the authors have to say about the Atlantic sea routes, and their very good reasons for believing that the first cauldrons reached Ireland and Britain by way of the Tin Route, or at least that part of it which crossed France linking

the Mediterranean with the western estuaries. The route by way of the Straits of Gibraltar was beyond the Greek sphere at this time, and perhaps quite unknown to them, and the Huelva hoard is only a good witness for maritime trade at a later period. This cargo of nicely mixed types, representative of Irish-British, Atlantic and Urnfield French, and central-east Mediterranean, provenances, bespeaks an early or mid-7th century date, and a more complex cultural background in the bronze trade outside the Straits than had appeared in the 8th century. The fibulae from the Huelva hoard point to the Phoenician trade interests in Sardinia, and a sphere of influence south-westwards therefrom.

Enough has been said to indicate the wide range and high scholarship of this paper, and of the still unfolding potentialities in European archaeology which in recent years have notably pushed back the ' Old Night ' of absolute prehistory. To this assault the authors have made a notable contribution.

T. G. E. POWELL

HISTORY OF THE BRITISH IRON AND STEEL INDUSTRY FROM *c.* 450 B.C. TO A.D. 1775. By H. R. Schubert. *Routledge and Kegan Paul. London*, 1957. *pp.* 445, 36 *text figures*, 26 *plates* and 11 *maps*. 60*s*.

This is an exceptional book, for it is probably the first time that the history of iron and steel in Britain, from the dawn of the prehistoric Iron Age to the introduction of relatively modern processes, has been given in such detail. The author divides his work into two parts, the first part dealing with the direct, and the second part with the indirect method for the production of iron. To readers of ANTIQUITY the first section will be of the greatest interest because it covers the old bloomery process by which wrought iron was produced directly from the ore in small furnaces. In time, this phase covers all the prehistoric Iron Age, and on into Medieval times.

In a general survey of the early iron industry the smelting furnace itself is naturally of first importance. Then follows the discovery of, and the place occupied by, steel within the general iron industry. Upon these fundamental themes Dr Schubert's long and detailed discussion is original and admirable. Concerning the furnace, perhaps the author has obtained all the important known evidence for prehistoric and early bloomery furnaces in this country. As a general picture it would seem that the predominant furnace type of the prehistoric and early industry was a very simple one. It appears that most iron production was from small bowl furnaces furnished with a bellows to provide draught in order to attain the desired temperature. Certainly from Roman times in Britain one would expect to find some of the more advanced furnace types which were known in Europe. However, there does not seem to be any real evidence for the use of more advanced types. Indeed, even so late as the 6th–9th centuries A.D., it is probable that the bowl furnace was in use, and it is by no means certain that it may not have continued on into even later times. Evidence for early furnaces is unfortunately rather scanty but, from Dr Schubert's discussion it would appear that from the prehistoric Iron Age, and at least to the Dark Ages, the simple bowl furnace was the favoured type for British production. On the other hand, but few furnaces have been found and well excavated in this country, so perhaps we have been unlucky in not finding the more advanced constructions recorded from Europe. At least, this has spared us possible confusion, and the dangers which may arise from too much typology.

To the archaeologist and general reader the question of steel within the iron industry may well appear complex and difficult to follow. Here, the student will be well repaid by careful reading of the author's ample and masterly exposition of the subject. Steel, or

steel-like material, may be produced in various ways, and even accidentally. We learn from Dr Schubert that steel can be produced directly from manganese bearing ores under specific smelting conditions. Again, by intentional or even accidential surface carburization of wrought iron; and also by the cementation process which, in its true application is a considered and relatively controlled technique. This matter of early steel is a fascinating one, over the time-span from Early Iron Age to Medieval we find variations in the quality of the material from a pure base wrought iron up to high carbon steel (as the high carbon steel of the Llyn Cerrig Bach chariot tyres), but it is difficult to discern any uniform pattern of metallurgical development. We must remember that the question is that of the position of steel, or steel-like material, within a general iron industry. Certainly, steel never supplanted iron in early days—we may recall that even in the 18th–19th centuries A.D., high quality wrought iron formed the backbone of our engineering industries. The author is to be congratulated for so clearly stating the case, with all the relevant evidence, for prehistoric and early steel. He gives an instructive table of analyses (Appendix I). However, before we can estimate the place and importance of so-called ' steel in antiquity ', we shall need many more analyses backed with metallographic research. Dr Schubert has stated the problem and carried it a considerable way. It now remains for iron technologists and archaeologists to complete the picture.

As Dr Schubert states, the blast furnace producing cast iron appeared in Britain between A.D. 1490 and 1500. The possibility that cast iron was in useful production before that time is an old question but, unfortunately, there is little evidence to go on. Indeed, there is hardly more to be said than the author's statement that, ' the possibility is admitted that some knowledge of it (cast iron) reached the Roman Empire from China where it was known much earlier '. It has sometimes been suggested that a number of European cast iron statuettes are of Roman date, but the supporting evidence is usually unsatisfactory. Certainly, the date of the Beauport Park (near Hastings) statuette is decidedly uncertain. In passing, it may be remarked that the analysis of this specimen is not due to the reviewer (see footnote 3 to p. 58), it was analysed many years ago by Dr Kelner at the Royal Arsenal, Woolwich and, I believe, lately by the British Cast Iron Research Association. This statuette is certainly made of cast iron, but its date cannot be established with any accuracy.

In a short review we can only mention some outstanding features of this substantial work, in which a considerable amount of new material is presented. As well as much valuable material concerning Forest of Dean and Wealden iron, there are excellent chapters upon Saxon and Norman iron, and in Chapter VI, ' Rise and Decline in the Medieval Renaissance ' will be found a very complete and valuable account of the mines and workshops of the industry during that period. Again, in the appendices the catalogue and documentation of blast furnaces, output of iron, and other matters of interest in the sphere of production, are outstanding and invaluable to the student. To those engaged upon a study of the later periods of the iron industry Chapter XV will be most helpful as it deals with cast iron for industrial, domestic, and decorative use. In other chapters the rise of the industry around the charcoal blast furnace, up to the transition from charcoal to coke as a metallurgical fuel, is discussed at length.

In conclusion, as we expect from the Historical Investigator to the British Iron and Steel Institute, the references and documentation throughout are most thorough, and of essential value. Without question, this book will take its place as a standard work of reference. It is not too much to say that it is one of the few books which are invaluable to the archaeologist and technologist engaged upon the study of ancient iron. It is indeed to be recommended.

H. H. Coghlan

RECEUIL DES BRONZES DE BAVAI. *By* Germaine Faider Feytmanns. (VIII[e] supplement à ' Gallia '.) *Centre National de la Recherche Scientifique. Paris*, 1957. *pp.* 140 + 57 *plates* + 2 *plans.*

This inventory of the bronzes of Bavai will be valuable to students of Roman provincial art. Bavai, as capital of the *civitas Nerviorum*, was an important administrative and commercial centre, and the wealthier provincials soon acquired a taste for the outward trappings of Mediterranean culture, including small *objets d'art* which they imported from Italy and Provence. In particular there was a keen demand for bronze statuettes, a demand that was apparently often satisfied by local craftsmen who manufactured rough and ready souvenirs of classical works which had already suffered time and again at the hands of earlier copyists. To supply a conservative and possibly more discriminating market were those bronzes which betray a greater sympathy with Celtic formalism than with the naturalistic traditions of the Mediterranean area. More interesting typologically, as presumably reflecting genuine taste as opposed to fashion, are the objects other than statuettes, such as key-grips, lock plates, chariot fittings and so on.

Bavai has been despoiled by successive generations of treasure seekers, so that the place and circumstances of finds is rarely known and was often deliberately concealed. Until recently there was no scientific excavation of the site, and an established chronology for most of the pieces is out of the question. Mme Faider Feytmanns is completely honest about this, and cautions the reader against the statements of Hénault, director of the excavations between 1923 and 1931, who ' ne controllait ses fouilles que le dimanche et receuillait ces jours-là les objets trouvés durant la semaine ecoulée par les ouvriers travaillant dans les sablières '.

Working at this considerable disadvantage, Mme Faider Feytmanns has tackled the problem in a thorough and scholarly manner. With great good humour—for clearly she loves her villains no less than her heroes—she gives in her informative introduction an account of the savants, the collectors and the rogues through whose hands the bronzes have passed at one time and another. Now dispersed in private collections, in the museums of Northern France and Belgium, and even as far afield as London, the bronzes of Bavai are reunited in this book. Not even the forgeries are omitted.

It is perhaps regrettable, but not very important, that few of the bronze statuettes, even of those probably imported, have much artistic merit. The British Museum Heracles (pl. IX) and the copy of Cresilas' Vulneratus Deficiens (?) (pls. XXIV, XXV) are highly competent works. The latter (*pace* the author) does not look to me like an Amazon, and accords well—so far as facial features are concerned—with what we know of Cresilas' style (cp. the British Museum Pericles, the Capitoline Amazon, and the Athena of Velletri). The so-called Érôs Bacchique (pl. XIV) is, as Mme Faider Feytmanns says, ' Pièce très gracieuse et pleine de mouvement ', but is presumably thought too obscene to have been widely published. The Gallo-Roman statuettes have the common defect of hybrids in falling between two stools. Of the native pieces, the Captive (pl. XXI, 94) is outstanding.

Individual and Celtic in spirit are the the two key-grips in the form of horses' heads (pl. XLII, 258, 259). So too is the drawer (?) handle with two opposing dolphins, though the arrangement is ultimately influenced by the Classical preference for symmetry.

The production of the plates is not worthy of the text, and it is unfortunate, though unavoidable, that so many should have to be reproductions of the drawings—' sans style mais avec précision '—made by the abbé Augustin Carlier between his arrival at Bavai in 1758 and his death in 1818. It is also aggravating to find that the numbering of objects on successive plates is often out of sequence.

Nevertheless, Mme Faider Feytmanns is to be congratulated on a readable book, as informative as is possible before the results of scientific excavation have elucidated the problems, chiefly those of chronology, that still confront her.

MICHAEL GOUGH

EXCAVATIONS AT GÖZLÜ KULE, TARSUS; From the Neolithic through the Bronze Age. *vol.* II. *Text, pp.* viii + 373; *vol.* II, *plates*, 460 *figures and* 26 *plans. By* HETTY GOLDMAN, with chapters by MACHTELD J. MELLINK and I. J. GELB, and an appendix by FREDERICK R. WATSON. *Princeton University Press* (*London, Geoffrey Cumberlege*), 1956. £14 8*s*.

These two luxurious volumes contain the final report of the American excavations in the prehistoric levels of the mound of Tarsus in Cilicia.

The book is a major contribution to Anatolian archaeology, for Tarsus has produced an almost complete record of the Bronze Age cultures in the Cilician plain. The expedition was able to ascertain the architectural character of the Bronze Age city through about twenty-eight successive building levels, covering the two later subdivisions of the Early and the whole of the Middle and Late Bronze Age, a period extending from about 2750–1100 B.C. Excessive depth prevented the excavations from continuing on the same large scale into the 12·5 m. thick earlier deposits and the sounding in which these were reached produced a good pottery sequence, but little else. Consequently, much remains to be learnt about these earlier periods.

Although the Neolithic remains are almost identical with those from Mersin, 30 kms. to the west, the chalcolithic ones are markedly different. Tarsus appears to represent the chalcolithic of the Cilician plain, whereas Mersin shows a fair number of links with other chalcolithic cultures, at home in western Cilicia and the Konya plain.

Although the discoveries at Tarsus are of absorbing interest, the presentation of the material does not facilitate reading. No synthesis is made and but for the tantalizingly short summary in chap. XVI, the material is undigested. Instead of presenting an account of the successive cultures in chronological order, the reader is faced with separate chapters on building-levels, pottery, stone, bronze, etc., each described with a suffocating wealth of detail.

The failure to number the building-levels is a continual source of annoyance and as no table is provided, the reader is compelled to work out the stratification on his own. The section through the mound (plan 26) is too confused to be of much help. Description of each building level by depth is particularly bad when applied to individual pots found in these levels, which are more often than not honeycombed with pits (see e.g. fig. 101). One wonders how pottery from a pit can be dated by such a system.

Tables illustrating the occurrence of pottery shapes by building level are conspicuously absent, and the reader is asked to work out the sequence (p. 4) from catalogues, unparalleled in archaeological literature for their superfluous detail, much of which could easily be gleaned from the illustrations (e.g. p. 125, no. 311).

This same tendency of over-elaboration is manifest in the description of the E.B. (I and II) pottery, where five or six classes of pottery are divided into not less than 16 E.B. I and 25 E.B. II wares, some of which are represented by not more than two fragments! (p. 105). This surely serves no serious purpose and cannot but confuse the reader. The tendency to quote parallels only in the catalogue is most inconvenient and fortunately not adopted in Miss Mellink's chapter.

The volume of plates shows a very large number of small scale excavation photographs, many of which record features, which could have been more adequately illustrated in a

drawing, such as e.g. the decorated hearths, figs. 135 and 159. The absence of a scale in many of these photographs is noticeable and many of the photographs are poorly reproduced.

A number of elevation drawings of the well preserved houses of the E.B. II period would not have been amiss. The excavators failed to grasp the opportunity of showing reconstructions of the city wall and adjacent quarters of the E.B. II city, the Tavern (p. 18), the E.B. III complexes of west Anatolian houses and the M.B.A. houses with central post and decorated hearth.

When more than one building level is shown on the plans, some legend as to which is which, should have been included.

The pottery drawings are good, those of the seal impressions exquisite, but a notable omission in the book is the lack of drawings illustrating the metal objects with their sections. This is particularly serious as many of these appear to have been photographed before cleaning.

Many household utensils of little or no importance are photographed in extenso, whereas, e.g. the crystal statuette (fig. 456), is only illustrated in four small photographs without a scale.

Many of the plans show a low standard of draughtsmanship, unworthy of the book, and the unsavoury section (*plan* 26) is unintelligible without the use of colour.

One cannot help feeling that this valuable reference book could have been produced at a fraction of the actual cost (£14 8*s.* 0*d.*) without omitting any essential material. The main bar to its widespread use is its exorbitant price.

JAMES MELLAART.

CITIES IN THE SAND; Leptis Magna and Sabratha in Roman Africa. *By* K. D. MATTHEWS, JR. and A. W. COOK. *Pennsylvania University Press; London: Oxford University Press*, 1957. *pp.* 56. 97 *plates.* 80*s.*

Mr Cook, who was stationed in Tripoli air base in 1951 as Chief Photographer for the U.S. Corps of Engineers, is a photographer of imagination and skill, and this book is certainly a ' visual lure ' as he and Mr Matthews hope, but it is curiously lopsided and, therefore, does not substantiate their claim of revealing the personality of the two cities. Of the ninety-seven plates, thirty-three—all superb photographs—are devoted to the theatres of Leptis and Sabratha; seventeen show miscellaneous statuary from the Leptis Museum (now mostly transferred to Tripoli Museum). Some of this art is beautiful, much is interesting, but a lot of the detail has been lost by unduly softened outlines. We are left with forty-seven plates for the rest of the sites. As a result the splendours of the Severan Forum and Basilica at Leptis do not get across adequately, except in the air-photographs, and important antiquities, such as the port, the circus, the great municipal cisterns and the hunting-baths, though they are all mentioned in the text, do not get a single picture. One of the charms of the Sabratha ruins is the number of houses that have been excavated: this domestic side of Sabratha is hardly represented. It is as if the photographer was so fascinated by the theatres and their immediate neighbourhood that he could hardly bear to move away from them.

The text is a factual summary of the history of Roman Africa and of the buildings in the two cities: the latter part would have been easier to visualize had a plan of some sort been provided. The book is beautifully produced; there is a small misprint on p. 41, line 26, where for Domitian we should read Commodus.

OLWEN BROGAN

THE PREHISTORY OF EASTERN EUROPE, PART I, MESOLITHIC NEOLITHIC AND COPPER AGE CULTURES IN RUSSIA AND THE BALTIC AREA. *By Marija Gimbutas. American School of Prehistoric Research, Bulletin no.* 20. *Cambridge, Mass.*, 1956. *p.* 241 *with* 50 *plates, and* 126 *text figures.* $7·50.

The interest of the American School of Prehistoric Research in the prehistory of Eastern Europe is well known. The archaeological reconnaissance of areas of Yugoslavia and the excavation of Starčevo under the direction of V. J. Fewkes and R. W. Ehrich was of first importance, and J. H. Gaul's *The Neolithic Period in Bulgaria* (Bulletin no. 16, 1948) was a definitive publication. Dr Gimbutas' work is intended as an introduction to the prehistory of the whole of Eastern Europe exclusive of the Balkans, from the Carpathians to the Urals and from the White Sea to the Caspian. Part I deals with the Mesolithic, Neolithic and Copper Ages, while two further parts, to deal with the Bronze and Iron Ages, are promised.

The series must be regarded as a major undertaking. Part I is, as one would expect, a synthesis of the work of many. It is, in fact, a well-balanced exposition of the present state of prehistoric research in Eastern Europe. Where Dr Gimbutas' own theories are expressed, they are clearly acknowledged as such, and while cautious, are at the same time constructive.

She follows the Russians in regarding the Shan Koba (' Pontic Azilian ') and Murzak Koba (' Pontic Tardenoisian ') cultures as essentially native developments brought about through changes of environment. The latter phase is followed by the ' Pontic Sub-Neolithic ', wherein pottery makes its appearance in connection with a microlithic flint assemblage of developed Tardenoisian type. At Igren on the Lower Dnieper, for example, material of this phase is found stratified above the Murzak Koba and below the local Neolithic, the flint work showing considerable continuity throughout. Similarly at several sites in the Crimea and at Terpin, north of the Sea of Azov, pottery occurs with ' Tardenoisian ' microliths stratified immediately above the Mesolithic levels, while a site near Zhitomir in Volhynia shows the same sub-Neolithic assemblage stratified between Mesolithic and Tripolye. These facts suggest the adoption of some of the Neolithic arts by a Mesolithic population in the North Caucasus and the Ukraine, and their discovery lends significance to recent finds of parallel phenomena in Greece, Yugoslavia and the west.

In the Neolithic Period proper, much of Eastern Europe is patently affected by influences from the West. The funnel-beaker culture of the South Baltic, the Danubian culture of Poland and Volhynia, and the painted pottery culture of Tripolye are not to be understood without detailed reference to Scandinavia, Central Europe and the Balkans respectively. Dr Gimbutas accordingly discusses the Czechoslovakian and Danish material, and touches upon the Becker, Troels-Smith controversy. With regard to the Balkan connections of Tripolye, however, one might have hoped for a more detailed treatment, especially in view of the previous work of the A.S.P.R. in the south-east. The earliest Tripolye seems to be related to the Vinča-Tordos-Boian A complexes, in spite of a probable local origin for the flint and bone industries.

On occasion, this book, considering its size, shows a curious lack of detail. The section on the Tripolye culture omits a description of the flints, although they ' allow the assumption that this Neolithic culture was locally developed '. In her discussion of the Danubian flint assemblages of Volhynia, however, Dr Gimbutas draws a comparison between these and the Tripolye assemblages. The Volhynian material includes burins. It is interesting to note that a small flint assemblage in the Cambridge Museum (England) from the famous Tripolye site of Niezwiska includes unpublished burins.

The regions east of the Dnieper and the North Caucasus were orientated not westwards but to the south, and there are links, indirect rather than direct, with the Early Helladic, Early Cycladic and Early Minoan cultures. A sub-division is proposed of Gorodtsov's *yamno* culture, into an earlier pit-grave phase and a later hut-grave phase in which copper artefacts appear.

The chapter on the final Neolithic is perhaps the most controversial. The Bell-Beaker material is passed over quickly, without reference to the old theory of a Central European origin, which cries out for revision. The original globular amphora spread, slightly anterior to the Beaker horizon, is explained as due to intrusive cultural elements from the Pontic regions where many of the new features occur already in the Kuban and *yamno* cultures, especially in the newly defined hut-grave phase: the globular amphora tradition survives in the various locally differentiated corded-ware-battle-axe groups. Dr Gimbutas' arguments are to be contrasted with those recently put forward by Sulimirski (P.P.S. XXI).

Unlike most modern authors (with, of course the notable exception of Childe!) Dr Gimbutas finds it worth while to be constructive as well as cautious on the surely legitimate subject of national origins; and, indeed, the view that the various branches of the Indo-European languages were differentiated as early as the beginning of the 2nd millennium is happily compatible with the recently demonstrated antiquity of primitive Greek.

The last chapter deals in detail with the hunters and fishers of the north-east.

Such a publication as this has a usefulness outside itself. Each chapter is liberally provided with a bibliography, and these provide an invaluable guide to a large body of Polish and Russian archaeological literature, the illustrations in which, at the least, are now made intelligible to those who, like the reviewer, do not know the Slav languages. Dr Gimbutas is to be congratulated.

AUDREY FURNESS OZANNE

Book Chronicle—*continued from p. 267*

A PICTURE HISTORY OF ARCHAEOLOGY. *By* C. W. CERAM. *pp.* 360, 310 *illustrations and* 16 *colour plates. London: Thames and Hudson*, 1958. 42*s.* A lavishly illustrated and extremely well produced successor to Ceram's *Gods, Graves and Scholars.* Ideal present for youthful enthusiasts in archaeology, though gives a one-sided account of the development of the subject.

TROY (Settlements VIIa, VIIb, and VIII): Volume IV. *In two parts, Part I* (*Text, pp.* 328), *Part II* (*Plates,* 380 *figures and photographs*). *By* C. W. BLEGEN, C. G. BOULTER, J. L. CASKEY, and M. RAWSON. *Princeton, the University Press*, 1958. 288*s. the set of two volumes.* This is the fourth and concluding volume in the series giving detailed reports on the excavations at Hisarlik between 1932–8 by the Archaeological Expedition of the University of Cincinnati.

FROM MYCENAE TO HOMER: a study in early Greek Literature and Art. *By* T. B. L. WEBSTER. *pp.* 312, 24 *plates. London: Methuen*, 1958. 30*s.*

THE WORLD OF CAVES. *By* ANTON LÜBKE. *pp.* 295, 24 *photographs. London: Weidenfeld and Nicolson*, 1958. 30*s.* A translation of the German *Geheimnisse des Unterirdischen.* A general book about caves and caving.

TOWN AND COUNTRY IN ROMAN BRITAIN. *By* A. L. RIVET. *pp.* 195, 9 *figures,* 1 *plate. London: Hutchinson*, 1958. 10*s.* 6*d.* THE MEGALITH BUILDERS OF WESTERN EUROPE. *By* GLYN DANIEL. *pp.* 142, 25 *text figures,* 8 *plates. London: Hutchinson*, 1958. 18*s.* The first two volumes in the Archaeology Section of the Hutchinson University Library edited by Professor C. F. C. Hawkes.

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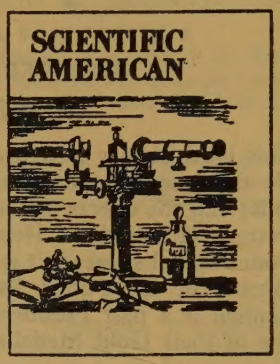

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